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NEW CONSTRUCTIONS IN GRAPHICAL STATICS.

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VI.

THE CONTINUOUS GIRDER WITH VARIABLE CROSS-SECTION.

In the foregoing articles the discussion of arches of various kinds has been shown to be dependent upon that of the straight girder; but as no discussion has, up to the present time, been published which treats the girder having a variable cross-section and moment of inertia, our discussion has been limited to the case of arches with a constant moment of inertia. Certain remarks were made, however, in the first article tending to show the close approximation of the results in case of a constant moment of inertia to those obtained when the moment of inertia is variable. We, in this article, propose a new solution of the continuous girder in the most general case of variable moment of inertia, the girder resting on piers having any different heights consistent with the limits of elasticity of the girder. This solution will verify the remarks made, and enable us easily to see the manner in which the variation of the moment of inertia affects the distribution of the bending moments, and by means of it the arch rib with variable moment of inertia can be treated directly.

Besides the importance of the continuous girder in case it constitutes the entire bridge by itself, we may remark that the continuous girder is peculiarly suited to serve as the stiffening truss of any arched bridge of several spans in which the arches are flexible. Indeed it is the conviction of the writer that the stiff arch rib adopted in the construction of the St. Louis Bridge was a costly mistake, and that, if a metal arch was desirable, a flexible arch rib with stiffening truss was far cheaper and in every way preferable.

Let us write the equation of deflections in the form

$$mD \cdot \frac{EI_0}{mn^2n'} = \Sigma\left(\frac{Mi}{nn'} \cdot \frac{x}{n}\right)$$

in which n is the number by which any horizontal dimension of the girder must be divided to obtain the corresponding dimension in the drawing, n' is the divisor by which force must be divided to obtain the length by which it is to be represented in the drawing, m is an arbitrary divisor which enables us to use such a pole distance for the second equilibrium polygon as may be most convenient, I_0 is the moment of inertia of the girder at any particular cross section assumed as a standard with which the values of I at other cross sections

are compared, and $i = I_0 \div I$ is the ratio of I_0 (the standard moment of inertia), to I (that at any other cross-section). For the purpose of demonstrating the general properties of girders, the equation need not be encumbered with the coefficients mnn', but for purposes of explaining the graphical construction they are very useful, and can be at once introduced into the equation when needed.

In the equation

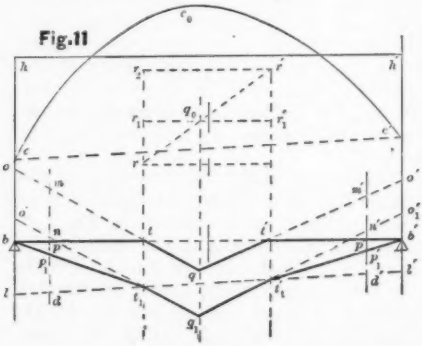
$$D \,.\, EI_0 = \Sigma_a^o (Mix)$$

the quantity D is the deflection of any point O of the girder below the tangent at the point a where the summation begins, and M is the actual bending moment at any point between O and a. These moments M at any point consist in general of three quantities, represented in the construction by the positive ordinate of the equilibrium polygon due to the weights, and by the two negative ordinates of the triangles into which we have divided the negative moment area. If we distinguish these components of M by letting M_0 represent that due to the weights, while M_1 and M_2 represent the components due to the left and right negative areas respectively, the equation of deflections becomes

$$D \,.\, EI_0 = \Sigma_a^o (M_0 ix) - \Sigma_a^o (M_1 ix) - \Sigma_a^o (M_2 ix)$$

Now let us take O at a pier at one end of a span and extend the summation over the entire span.

Fig. 11



If the piers are b and b' as in Fig. 11, let us suppose that O coincides with b and a with b'; also suppose for the instant that I is constant, so that $i = 1$ at all points of the girder. Then we have

$$D_b \,.\, EI = \bar{x}_0 \, \Sigma_{b'}^b (M_0) - \bar{x}_1 \, \Sigma_{b'}^b (M_1) - \bar{x}_2 \, \Sigma_{b'}^b (M_2)$$

in which D_b is the deflection of b below the tangent at b', x_0 is the distance of the center of gravity of the moment area due to the applied weights from b, while x_1 and x_2 are the distances of the centers of gravity of the negative areas from b. In Fig. 11 let cc_0c' be the positive area due to the weights and representing $\Sigma_{b'}^b (M_0)$, while $\Sigma_{b'}^b (M_1)$ and $\Sigma_{b'}^b (M_2)$ are represented by hcc' and $hh'c'$ respectively. Let the center of gravity of cc_0c' be in qq_0, while the centers of the two negative areas are in tr and $t'r'$. Let the height of a triangle on some assumed base, and equivalent in area to cc_0c', be rr_2, then by a process like that in Fig. 2 it is evident that rr_1 and r_1r_2 are the heights of the right and left negative triangles, having the assumed base, on the supposition that the girder is fixed horizontally over the piers.

Now introducing the constants mnn' into the last equation and into the equation before that, the relation of the quantities is such that if the moments be applied as weights at their centers of gravity with the pole distance $pt = EI \div mn^2n'$, the equilibrium polygon so obtained will be tangent at the piers to the exaggerated deflection curve obtained when the distributed moments are used as weights; and the deflection at the pier b from the tangent at b' will be the same as that of this exaggerated deflection curve, and vice versa.

Let $pm = r_1r_2$, $p'm' = rr_1$ and $pt = p't$, then t and t' constitute the pole, pm and $p'm'$ the negative loads, and $pm + p'm'$ the positive load. Then is $btqt'b'$ the equilibrium polygon for these loads. The deflection of b below $b't'$ vanishes as it should in case the girder is fixed horizontally over the pier.

Now let the direction of the tangents at the piers be changed so that the tangents to the exaggerated deflection curve assume the directions bt_1 and $b't_1'$. Then the load line and force polygon assume a new position, such that t_1 and t_1' form the pole, and $dn = pm$ and $d'n' = p'm'$ comprise the positive load while np_1 and $n'p_1'$ are the new negative loads

which will cause the equilibrium polygon $bt_1q_1t_1'b'$, which is due to them, to have its sides bt_1 and $b't_1'$ in the directions assumed.

There are several relations of quantities in this figure to which we wish to direct attention. It is evident that from the area cc_0c' whose ordinates are proportional to M_0, the actual bending moments due to the weights, another area whose ordinates are proportional to M_0i, the effective bending moments, can be obtained by simple multiplication, since i is known at every point of the girder. Moreover, the vertical through the center of gravity of this positive effective moment area can be as readily found as that through the actual positive moment area. Call this vertical "the positive center vertical." Again, the negative moment areas proportional to M_1i and M_2i can be found from the triangular areas proportional to M_1 and M_2 by simple multiplication, and if we proceed to find the verticals through their centers of gravity we shall obtain the same verticals whatever be the magnitude of the negative triangular areas, since their vertical ordinates are all changed in the same ratio by assuming the negative areas differently. Let us call these verticals the "left" and "right" verticals of the span. In case $i=1$, as in Fig. 11, the left and right verticals divide the span at the one-third points. This matter will be treated more fully in connection with Fig. 13.

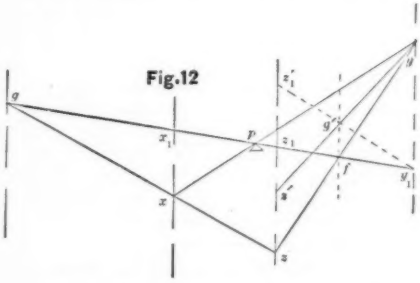


Fig.12

Again, let us call the line t_1t_1' "the third closing line." It is seen that, whatever may be the various positions of the tangent bt_1, the ordinate dn; between the third closing line and t_1q_1 prolonged, is invariable, for the triangle $t_1q_1t_1'$ is invariable, being dependent on the positive load and pole distance alone. By similarity of triangles it then follows that the ordinate, such as lo', on any assumed vertical continues invariable; and when there is no negative load at t_1, then bt_1q_1 becomes straight, o' coincides with b and n_1 with p_1. Similar relations hold at the right of q_1. The quantity dp_1 is of the nature of a correction to be subtracted from the negative moment when the girder is fixed horizontally at the piers in order to find the negative moment when the tangent assumes a new position, for $np_1=dn-dp_1$. The negative moments can then be found from the third closing line and the tangents at the piers; while the remaining lines q_1t_1 and $q_1't_1'$ will test the correctness of the work. Before applying these properties of the deflection polygon and its third closing line to a continuous girder, it is necessary to prove a geometrical theorem from Fig. 12.

Let the variable triangle xyz be such that the side xz always passes through the fixed point g, the side xy always passes through the fixed point p, and the vertices xyz are always in the verticals through those points; then by the properties of homologous triangles the side yz also has a fixed point f in the straight line gp. Furthermore, if there is a point z' in the vertical through z, and in all positions of z it is at the same constant distance from z, then on the line yz' there is a fixed point g' where the vertical through f intersects yz'; for, if z' maintains its distance zz' invariable, then must any other point as g' remain constantly at the same vertical distance from f, as appears from similarity of triangles. But as f is fixed g' is also. When, for instance, the triangle xyz assumes the position $x_1y_1z_1$, then z' moves to z_1'

Let us now apply the foregoing to the discussion of a continuous girder over three piers $p''pp'$ as shown in Fig. 13, in which the lengths of the spans have the ratio to each other of 2 to 3. Divide the total length of the girder into such a number of equal parts or panels, say 15, that one division shall fall at the intermediate pier, and let the number of lines in any panel of the type aa represent its relative moment of inertia. Assume the moment of inertia where there are three lines, as at a, a_3 etc., as the standard or

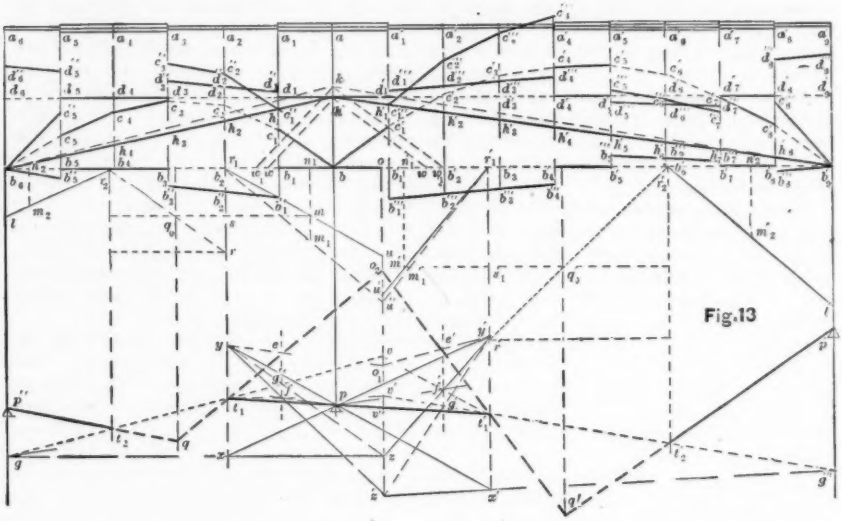


Fig. 13

I_0, then $i=1$ at a, $i=\frac{3}{4}$ at a_2, $i=\frac{1}{2}$ at a_4', etc.

Let the polygons c and c' be those due to the weights in the left and right spans respectively. There the ordinates of the type bc are proportional to M_0 in the left span. The figure $bc_1c_1''c_2''c_2'''c_3''c_3c_4c_5c_6''b_6$ is the positive effective moment area in the left span, and its ordinates are proportional to M_0i. Its center of gravity has been found, by an equilibrium polygon not drawn, to lie in the positive center vertical qq_0. A similar positive effective moment area on the right has its center of gravity in the positive center vertical $q'q_0'$.

Now assume any negative area, as that included between the lines b and d, and draw the lines hb_6 and hb_9', dividing the negative area in each span into right and left triangular areas. Let the quantities of the type hb be proportional to M_1, hd to M_2, $h'b'$ to M_1', etc., then the ordinates of $bb_1b_1''b_2''b_2'''b_3b_5b_5''b_6h$ are proportional to M_1i, and the center of gravity of this area has been found to lie in the right negative vertical t_1r_1. Similarly, the left negative vertical containing the center of gravity of the left negative effective moments, is t_2r_2. In the right span $t_1'r_1'$ and $t_2'r_2'$ are the left and right verticals. As before stated, these verticals would not be changed in position by changing the position in any manner whatever of the line d by which the negative moments were assumed, for such change of position would change all the ordinates in the same ratio.

Let us find also the vertical containing the center of gravity of the effective moment area, corresponding to the actual moment area b_9hb_9'. It is found by a polygon not drawn to be vo. Call vo "the negative center vertical." It is unchanged by moving the line d. If a polygon be drawn due to the effective moments as loads, two of its sides must intersect on vo, because it contains the center of gravity of contiguous loads. Now let rr' represent $\Sigma(M_0i)$:—it is in fact one eighth of the sum of the ordinates $b_1c_1+b_1c_1''$, etc., and hence is the height of a triangle having a base$=\frac{4}{3}bb_6$, and an area equal to the effective moment area in the left span. Also $r'r_1'$ is the height of a triangle having the same base, and an area equal to the effective moment area in the right span.

As previously explained, sr_1 is the amount of the right negative effective moment area in the left span, measured in the same manner, while sr is that on the left when the girder is fixed horizontally at the piers. We obtain $s'r_1'$ and $s'r'$ in the right span, in a similar manner. Now assume the arbitrary divisor $m=1$, and take the pole distance $r_1n_1=EI_0\div n^2n'$. Then as seen previously, if $mn_1=sr_1$,

ou is the constant intercept on the negative center vertical, between the third closing line in the left span, and a side of the type qt. Also ou' is a similar constant intercept on this vertical due to the right span. Make $r_2n_2=r_1n_1$ and $n_2m_2=sr$, then lb_6 is a similar invariable intercept; as is $l'b_6'$, which is obtained in a similar manner.

Now the negative center vertical ov was obtained from the triangle b_6hb_6', *i.e.* on the supposition that the actual moment over the pier is the same whether it be determined from the left or right of the pier. It is evident that while the girder is fixed horizontally at the intermediate pier, the moment at that pier is generally different on the two sides, at points infinitesimally near to it, but that when the constraint is removed an equalization takes place.

Since ou and ou' are derived from the positive effective moments, it appears that when the tangent at p is in such a position that the two third closing lines intercept a distance uu' on ov and the two lines of the type qt when prolonged intersect on ov, the moments over the pier will have become equalized.

We propose to determine the position of the tangent at p which will cause this to be true, by finding the proper position of the third closing lines in the two spans.

Move the invariable intercepts to a more convenient position, by making $o_1z=ou$, and $o_1z'=ou'$. Now by making the arbitrary divisor $m=1$, as we did, the ordinates of the deflection polygon became simply D, *i.e.*, they are of the same size in the drawing as in the girder, hence the difference of level of p'', p and p' must be made of the actual size. By changing m this can be increased or diminished at will.

Now we propose to determine two fixed points g and g'', through which the third closing line in the left span must pass, and similarly g''' and g' on the right.

If the girder is free at p'' then as shown in connection with Fig. 11, the third closing line must pass through g, if $gp''=lb_6$. Draw gz as a tentative position of the third closing line, and complete the triangle $xy'z$ as in Fig. 12.

Then is xy' the tentative position of the tangent at p, and since the third closing line in the right span must pass through y', and make an intercept on the negative center vertical equal to uu', then $z'y'$ is its corresponding tentative position. But wherever gz may be drawn, every line making an intercept $=uu'$ and intersecting $t_1'r_1'$ in such a manner that the tangent passes through p must pass through the fixed point g', found as described in Fig. 12. Therefore the third closing line in the right span passes through g'. Similarly, if there were more spans still at the right of these, we should use g' for the determination of another fixed point, as we have used g to determine it.

Now find g''' and g'' precisely as g and g' have been found, and draw the third closing lines t_1t_2 and $t_1't_2'$. If t_1t_1' passes through p the construction is accurate. Make $uu''=vv''$, then is n_1m_1 the negative effective moment at the left, and $n_1'm_1'$ that at the right of the pier.

Let bw be the effective moment area corresponding to the triangle hbb_6, and measured in the same manner as the positive area was, by taking one eighth of its ordinates, and let $bw_1=n_1m_1$; then as the effective moment bw is to the actual moment bh corresponding to it, so is the effective moment bw_1 or n_1m_1 to the actual moment bk corresponding to it. The same moment bk is also found from $n_1'm_1'$, by an analogous construction at the right of b, which tests the accuracy of the work.

Several other tests remain which we will briefly mention.

Prolong $p''t_2$ to q, and $p't_2'$ to q', then qt_1 and $q't_1'$ must intersect on the negative center vertical at o_2 so that $o_2v''=ou''$. Also vv' must be equal to uu'. Again t_1v' passes through f, and $t_1'v$ through f'. Also yo_2 intersects qo_2 on the fixed vertical fg'' at e, and $y'o_2$ intersects $q'o_2$ on the fixed vertical $f'g'$ at e'. That these must be so is evident from a consideration of what occurs during a supposed revolution of the tangent t_1t_1', to the position xy'.

Now having determined the moment bk over the pier, kb_6 and kb_6' are the true closing lines of the moment polygons c and c'. Call these closing lines k, then the ordinates of the type kc will represent the bending moments at different points of the girder. The points of the contra flexure are at the points where the closing lines inter-

sect the polygons c and c'. The directions of the closing lines will permit at once the determination of the resistances at the piers and the shearing stresses at any point.

The particular difference between the construction in case of constant and of variable moment of inertia, is seen to be in the positions of the center verticals positive and negative, and the right and left verticals.

The small change in their position due to the variation in moment of inertia, is the justification of the remarks previously made respecting the close approximation of the two cases.

It is seen that the process here developed can be applied with equal facility to a girder with any number of spans. Also if the moment of inertia varies continuously instead of suddenly, as assumed in Fig. 13, the panels can be taken short enough to approximate with any required degree of accuracy to this case.

THE THEOREM OF THREE MOMENTS.

The preceding construction has been in reality founded on the theorem of three moments, but when the equation expressing that theorem is written in the usual manner, the relationship is difficult to see. Indeed the equation as given by Weyrauch* for the girder having a variable moment of inertia, is of so complicated a nature that it may be thought hopeless to attempt to associate mechanical ideas with the terms of the equation, in any clearly defined relationship. We propose to derive and express the equation in a novel manner, which will at once be easy to understand, and not difficult of interpretation in connection with the preceding construction.

Let us assume the general equation of deflections in the form.

$$D=\Sigma(Mx\div EI), \text{ or } D.EI_0=\Sigma(Mix) \quad (7)$$

in which I is the variable moment of inertia, I_0 some particular value of I assumed as the standard of comparison, $i=I_0\div I$, and x is measured horizontally from the point as origin, where the deflection D is taken to the point of application of the actual bending moment M. The quantity Mi is called the effective bending moment, and the deflection D is measured from the tangent of the deflection curve at the point to which the summation is extended from the origin.

Now consider two contiguous spans of a continuous girder of several spans, and let acb denote the piers, c being the intermediate pier. Let the span $ac=l$ and $bc=l'$. Take the origin at a and extend the summation to c, calling the deflection at a, D_a. When the origin is at b and the summation extends to c, let the deflection be D_b. Let also y_a,y_b and y_c be the heights of a, b and c respectively above some datum level. Then, as may be readily seen,

$$D_a = y_a - y_c - lt_c ,$$
$$D_b = y_b - y_c - l't_c',$$

if t_c is the tangent of the acute angle at c on the side towards a between the tangent line of the deflection curve at c and the horizontal, and t_c' is the tangent of the corresponding acute angle on the side of c towards b.

Now if we consider equation (7) to refer to the span l, the moment M may be taken to be made up of three parts, viz:—M_0 caused by the weights on the girder, M_1 dependent on the moment M_c at c, and M_2 dependent on the moment M_a at a. The moments in the span l' may be resolved in a similar manner. We may then write the equations of deflections in the two spans when the summation extends over each entire span as follows:

$$EI_0(y_a-y_c-lt_c)=\Sigma_c^a(M_0ix)-\Sigma_c^a(M_1ix) - \Sigma_c^a(M_2ix) \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$$EI_0(y_b-y_c-l't_c')=\Sigma_c^b(M_0'i'x') - \Sigma_c^b(M_1'i'x')-\Sigma_c^b(M_2'i'x') \quad (9)$$

in which x is measured from a, and x' from b towards c. Now if the girder is originally straight, $t_c = -t_c'$, hence we can combine these two equations so as to eliminate t_c and t_c', and the resulting equation will express a relationship between the heights of the piers, the bending moments (positive and negative), their points of application and the moments of inertia; of which quantities the negative bending moments are alone unknown. The equation we should thus obtain would be the general equation of which the ordinary expression of the

* Allgemeine Theorie und Berechnung der Continuirlichen und Einfachen Trager. Jakob I. Weyrauch. Leipzig 1873.

theorem of three moments is a particular case. Before we write this general equation it is desirable to introduce certain modifications of form which do not diminish its generality. Suppose that

$$\bar{x}_1 \Sigma_c^a (M_1 i) = \Sigma_c^a (M_1 i x)$$

then is $\bar{x}_1$ the distance from a to the center of gravity of the negative effective moment area next to c. As was shown in connection with Fig. 13, the position of this center of gravity is independent of the magnitude of M_1 or M_c and may be found from the equation,

$$\bar{x}_1 = \frac{\int_c^a i x^2 dx}{\int_c^a i x dx} \quad . \quad . \quad . \quad . \quad (10)$$

for M_1 is proportional to x. Similarly it may be shown that

$$\bar{x}_2 = \frac{\int_c^a i(l-x) x dx}{\int_c^a i(lx) dx} \quad . \quad . \quad (11)$$

is the distance of the center of gravity of the negative effective moment area next to a.

Again, suppose that

$$i_1 \Sigma_c^a (M_1) = \Sigma_c^a (Mi)$$

then is i_1 an average value of i for the negative effective moment area next to c, which is likewise independent of the magnitude of M_1, as appears from reasoning like that just adduced respecting $\bar{x}_1$. Hence i_1 may be found from the equation

$$i_1 = \frac{\int_c^a i x dx}{\int_c^a x dx} \quad . \quad . \quad . \quad (12)$$

Similarly it may be shown that

$$i_2 = \frac{\int_c^a i(l-x) dx}{\int_c^a (l-x) dx} \quad . \quad . \quad (13)$$

in which i_2 is the average value of i for the negative effective moment area next to a.

The integrals in equations (10), (11), (12), (13), and in others like them referring to the span l', which contain i must be integrated differently, in case i is discontinuous, as it usually is in a truss, from the case where i varies continuously. When i is discontinuous the integral extending from c to a must be separated into the sum of several integrals, each of which must extend over that portion of the span l in which i varies continuously.

Further more we have

$$\Sigma_c^a (M_1) = \tfrac{1}{2} M_c l \quad . \quad . \quad . \quad (14)$$

since each member of this equation represents the negative actual moment area next to c in the span l.

Similarly, we have the equations

$$\Sigma_c^a (M_2) = \tfrac{1}{2} M_a l \ , \ \Sigma_c^b (M_1') = \tfrac{1}{2} M_c' l',$$
$$\Sigma_c^b (M_2' l') = \tfrac{1}{2} M_b l').$$

If there is no constraint at the pier then must $M_c = M_c'$.

Now making the substitutions in equations (8) and (9), which have been indicated in the developments just completed, and then eliminating t_c and t_c',

$$EI_0 \left\{ \frac{y_a - y_c}{l} + \frac{y_b - y_c}{l'} \right\} - \frac{\bar{x}_0 i_0}{l'} \Sigma_c^a (M_0) -$$
$$\frac{\bar{x}_0' i_0'}{l'} \Sigma_c^b (M_0') = \tfrac{1}{2} [M_a \bar{x}_2 i_2 + M_c (\bar{x}_1 i_1 + x_1' i_1')$$
$$+ M_b \bar{x}_2' i_2'] \quad . \quad . \quad . \quad (15)$$

in which $\bar{x}_0$ is the distance from a of the center of gravity of the positive effective moment area due to the weights in the span l, and $\bar{x}_0'$ is a similar distance from b in the span l', while i_0 and i_0' are average values of i for these areas derived from the equations in each span,

$$i_0 = \Sigma (M_0 i) \div \Sigma (M_0).$$

It may frequently be best to leave the expressions containing the positive moments in their original form as expressed in equations (8) and (9).

Let us now derive from equation (15), the ordinary equation expressing the theorem of three moments, for a girder having a constant cross section. In this case $i = 1$, and we wish to find the value of the term $\Sigma (M_0 x)$ in each span. Let M_0 be caused by several weights P applied at distances z from a, then the moment due to a single weight P at its point of application is

$$M_z = Pz(l-z) \div l,$$

which may be taken as the height of the triangular moment area whose base is l which is caused by P. This triangle

whose area is $\frac{1}{2}M_o l$ is the component of $\Sigma(M_o)$ due to P and can be applied as a concentrated bending moment at its center of gravity at a distance x from a.

Now $x=\frac{1}{3}(l+z)$, and taking all the weights P at once

$$\Sigma_c^a(M_o x)=\tfrac{1}{6}\Sigma_c^a[P(l^2-z^2)z].$$

Also in equation (15) we have in this case

$$\bar{x}_1=\tfrac{1}{3}l,\ \bar{x}_2=\tfrac{2}{3}l,\ \bar{x}_1'=\tfrac{1}{3}l',\ \bar{x}_2'=\tfrac{2}{3}l'$$

$$\therefore\ 6EI\left\{\frac{y_a-y_c}{l}+\frac{y_b-y_c}{l'}\right\}$$

$$-\frac{1}{l}\Sigma_c^a[P(l^2-z^2)z]-\frac{1}{l'}\Sigma_c^b[P'(l'^2-z'^2)z']$$

$$=M_a l+2M_c(l+l')+M_b l' \quad . \quad (16)$$

Equation (16) then expresses the theorem of three moments for a girder having a constant moment of inertia I, and deflected by weights applied in the span l at distances z from a, and also by weights in the span l' at distances z' from b.

Let us also take the particular case of equation (15) when the moment of inertia is invariable and the piers on a level; then $i=1$, and if we let A_o and A_o' be the positive moment areas due to the weights we have

$$6\left\{\frac{1}{l}A_o\bar{x}_o+\frac{1}{l'}A_o'\bar{x}_o'\right\}=$$

$$M_a l+2M_c(l+l')+M_b l' \quad . \quad . \quad (17)$$

This form of the equation of three moments was first given by Greene.* The advantage to be derived in discussing this theorem in terms of the bending moments, instead of the applied weights is evident both in the analytical and the graphical treatment. The extreme complexity of the ordinary formulae arises from their being obtained in terms of the weights.

In order to complete the analytic solution of the continuous girder in the general case of equation (15), it is only necessary to use the well known equations,

$$M=M_c+S_o z_o-\Sigma_c^o(Pz_o) \quad . \quad . \quad (18)$$

$$S_o=\frac{1}{l}[M_a-M_c+\Sigma_c^a(Pz)] \quad . \quad . \quad (19)$$

$$S_c'=\frac{1}{l'}[M_b-M_c+\Sigma_c^b(Pz')] \quad . \quad (20)$$

$$R_c=S_c+S_c' \quad . \quad . \quad . \quad . \quad . \quad (21)$$

$$S=S_c-\Sigma_c^o(P) \quad . \quad . \quad . \quad . \quad (22)$$

In (18) M is the bending moment at any point O in the span l, S_c is the shear at c due to the weights in the span l, and z_o is the distance from O towards c of the applied forces P and S_c in the segment Oc.

Equation (19) is derived from (18) by taking O at a, and (20) is obtained similarly in the span l'. R_c is the reaction of the pier at c. S is the shear at O in the span l. These equations also complete the solution of the cases treated in (16) and (17).

THE FLEXIBLE ARCH RIB AND STIFFENING TRUSS.

Whenever the moment of inertia of an arch rib is so small, that it cannot afford a sufficient resistance to hold in equilibrium the bending moments due to the weights, it may be termed a flexible rib.

It must have a sufficient cross section to resist the compression directly along the rib, but needs to be stiffened by a truss, which will most conveniently be made straight and horizontal. The rib may have a large number of hinge joints which must be rigidly connected with the truss, usually by vertical parts. It is then perfectly flexible.

If, however, the rib be continuous without joints, or have blockwork joints, it may nevertheless be treated as if perfectly flexible, as this supposition will be approximately correct and on the side of safety, for the bending moments induced in the truss will be very nearly as great as if the rib were perfectly flexible, in case the same weight would cause a much greater deflection in the rib than in the truss. It will be sufficient to describe the construction for the flexible rib without a figure, as the construction can afford no difficulties after the constructions already given have been mastered.

Lay off on some assumed scale the applied weights as a load line, and let us call this vertical load line ww'. Divide the span into some convenient number of equal parts by verticals, which will divide the curve a of the rib into segments. From some point b as a pole draw a pencil of rays parallel to the segments of a, and across this pencil

* Graphical Method for the Analysis of Bridge Trusses. Chas. E. Greene. Published by D. Van Nostrand. New York, 1875.

draw a vertical line uu', at such a distance from b that the distance uu' between the extreme rays of the pencil is equal to ww'. Then the segments of uu' made by the rays of the pencil are the loads which the arch rib would sustain in virtue of its being an equilibrium polygon, and they would induce no bending moments if applied to the arch. The actual loads in general are differently distributed. By Prop. VI the bending moments induced in the truss are those due to the difference of the weight actually resting on the arch at each point, and the weight of the same total amount distributed as shown by the segments of the line uu'.

Now lay off a load line vv' made up of weights which are these differences of the segments of uu' and ww', taking care to observe the signs of these differences. The algebraic sum of all the weights vv' vanishes when the weights which rest on the piers are included, as appears from inspection of the construction in the lower part of Fig. 10. The construction above described will differ from that in Fig. 10 in one particular. The rib will not in general be parabolic, and the loads which it will sustain in virtue of its being an equilibrium polygon will not be uniformly distributed, hence the differences which are found as the loading of the stiffening truss do not generally constitute a uniformly distributed load.

The horizontal thrust of the arch is the distance of uu' from b measured on the scale on which the loads are laid off, and the thrust along the arch at any point is length of the corresponding ray of the pencil between b and uu'. These thrusts depend only on the total weight sustained, while the bending moments of the stiffening truss depend on the manner in which it is distributed, and on the shape of the arch.

Having determined thus the weights applied to the stiffening truss, it is to be treated as a straight girder, by methods previously explained according to the way in which it is supported at the piers.

The effect of variations of temperature is to make the crown of the arch rise and fall by an amount which can be readily determined with sufficient exactness, (see Rankine's Applied Mechanics Art. 169). This rise or fall of the arch produces bending moments in the stiffening truss, which is fastened to the tops of the piers, which are the same as would be produced by a positive or negative loading, causing the same deflection at the center and distributed in the same manner as the segments of uu': for it is such a distribution of loads or pressures which the rib can sustain or produce. A similar set of moments can be induced in the stiffening truss by lengthening the posts between the rib and truss.

When this deflection and the value of EI in the truss are known, these moments can be at once constructed by methods like those already employed. A judicious amount of cambering of this kind is of great use in giving the structure what may be called "initial stiffness." The St. Louis Arch is wanting in initial stiffness to such an extent that the weight of a single person is sufficient to cause a considerable tremor over an entire span. This would not have been possible had the bridge consisted of an arch stiffened by a truss which was anchored to the piers in such a state of bending tension as to exert considerable pressure upon the arch. This tension of the truss would be relieved to some extent during the passage of a live load.

The arch rib with stiffening truss, is a form of which many wooden bridges were erected in Pennsylvania in the earlier days of American railroad building, but its theory does not seem to have been well understood by all who erected them, as the stiffening truss was itself usually made strong enough to bear the applied weights, and the arch was added for additional security and stiffness, while instead of anchoring the truss to the piers and causing it to exert a pressure on the arch, a far different distribution of pressures was adopted. Quite a number of bridges of this pattern are figured by Haupt* from the designs of the builders, but most of them show by the manner of bracing near the piers that the engineers who designed them did not know how to take advantage of the peculiarities of this combination. This further appears from the fact, that, the trussing is not usually continuous.

* Theory of Bridge Construction. Herman Haupt, A.M. New York. 1853.

A good example, however, of this combination constructed on correct principles is very fully described by Haupt on pages 169 *et seq.* of his treatise. It is a wooden bridge over the Susquehanna River, 5½ miles from Harrisburg on the Pennsylvania Railroad, and was designed by Haupt. It consists of twenty-three spans of 160 feet each from center to center of piers. The arches have each a span of 149¼ feet and a rise of 20 ft. 10 in., and are stiffened by a Howe Truss which is continuous over the piers and fastened to them. It was erected in 1849. Those parts which were protected from the weather have remained intact, while other parts have been replaced, as often as they have decayed, by pieces of the original dimensions. This bridge, though not designed for the heavy traffic of these days, still stands after twenty-eight years of use, a proof of the real value of this kind of combination in bridge building.

STOPPING AND STARTING RAILROAD TRAINS.

By Prof. E. W. HYDE, C.E., University of Cincinnati.

Written for Van Nostrand's Magazine.

It is proposed in this paper to compare the expense of stopping a train moving at a given speed, and then bringing it up again to that speed, with that of maintaining the given velocity unchanged over the distance between the point at which steam is shut off, for stopping the train, and the point at which the original velocity is again attained.

The main thing to be considered is of course the amount of mechanical work done in each of the two cases.

To determine this it will be necessary to investigate the relations existing between time, space, and velocity for a moving train.

The resistances encountered by the train are in part constant, and in part dependent on the velocity. Several empirical formulæ have been given expressing more or less accurately the amount of these resistances. Mr. Scott Russell has found for the train, exclusive of the locomotive, resistance in lbs. $= \left(6+\frac{V}{3}\right)T+\frac{V^2A}{400}$, in which T = weight of train in tons, V = velocity in miles per hour, and A = area of frontage in square feet. Mr. Wyndham Harding gives for the same, resistance $= \left(6+\frac{V}{15}\right)T+\frac{V^2B}{50000}$, in which V and T are as before, and B is the volume of the train in cubic feet.

Mr. Gooch gives

$$\text{resistance} = 6T\left(1+\frac{V-10}{20}\right);$$

and Mr. D. R. Clark

$$\text{resistance} = 6T\left(1+\frac{V^2}{1440}\right).$$

These formulæ may be found in Rankine's Civil Engineering. Rankine also gives a formula for the locomotive and train as follows:

$$\text{total resistance} = R = (T+E)\left(8+\frac{V^2}{180}\right),$$

E being the weight of the engine in tons.

All these formulæ apply to level track only, and in what follows we shall always suppose a level, straight track, and no wind blowing so as to have an appreciable component in the direction of the road.

We may write in general

$$R=F+\mu_1 v+\mu_2 v^2 \quad . \quad . \quad (1)$$

which will reduce to either of the above formulæ by giving proper values to F, μ_1, and μ_2. Hence if we call P_1 the accelerating force exerted by the locomotive, or the retarding force of the brakes, as the case may be, and M the entire mass of the train, we shall have

$$M\frac{d^2s}{dt^2}=M\frac{dv}{dt}=P_1\pm F\pm\mu_1 v\pm\mu_2 v^2 \quad . \quad (2)$$

We will first use the lower signs, making $P=P_1-F$.

$$\therefore\ t=M\int_0^v \frac{dv}{P-\mu_1 v-\mu_2 v^2}$$

$$=\frac{M}{a}\log.\frac{2P-v(\mu_1-a)}{2P-v(\mu_1+a)} \quad (3)$$

in which log. = Naperian logarithm, and

$$a=\sqrt{\mu_1^2+4\mu_2 P}.$$

$$\therefore \varepsilon^{\frac{at}{M}}=\frac{2P-v(\mu_1-a)}{2P-v(\mu_1+a)}+\varepsilon^{mt}, \text{ if we place } \frac{a}{M}=m;$$ whence solving for v, we have

$$v=\frac{ds}{dt}=\frac{2P(\varepsilon^{mt}-1)}{(\mu_1+a)\varepsilon^{mt}+a-\mu_1} \quad . \quad . \quad (4)$$

$$\therefore \quad s=2P\int_0^t \frac{(\varepsilon^{mt}-1)\,dt}{(a+\mu_1)\varepsilon^{mt}+(a-\mu_1)} \quad (5)$$

Let $\varepsilon^{mt}=x$; $\therefore t=\frac{1}{m}$ log. x, and $\frac{dt}{dx}=\frac{1}{m}.\frac{1}{x}$. Substituting in (5)) we have

$$s=\frac{2P}{m}\int_1^{\varepsilon^{mt}} \frac{(x-1)\,dx}{[(a+\mu_1)x+a-\mu_1]x}$$

$$\therefore s=\frac{2PM}{a}\left\{\frac{1}{a+\mu_1}\log.[(a+\mu_1)x+a-\mu_1]+\frac{1}{a-\mu_1}\log.\frac{(a+\mu_1)x+a-\mu_1}{x}\right\}_1^{\varepsilon^{mt}}$$

Whence by introduction of limits and reduction

$$s=\frac{M}{\mu_2}\log.\frac{(a+\mu_1)\varepsilon^{\frac{at}{M}}+a-\mu_1}{2a}-\frac{2Pt}{a-\mu_1} \quad (6)$$

Thus we have in equation (3) the relation between t and v, and in equation (6) that between s and t, for the case in which the velocity is accelerated from o up to v. If we substitute the value of t from (3) in (6), we obtain a relation between s and v as follows,

$$s=\frac{M}{\mu_2}\log\frac{2P}{2P-v(a+\mu_1)}-\frac{2PM}{a(a-\mu_1)}\log\frac{2P-v(\mu_1-a)}{2P-v(\mu_1+a)} \quad . \quad (7)$$

For the case in which the train is retarded, let $P'=P_1+F$, and

$$b=\sqrt{\mu_1^2-4\mu_2P'},$$

then changing the signs of μ_1 and μ_2 in the equations (3) (6) and (7) already obtained, we shall have

$$t'=\frac{M}{b}\log\frac{2P'+v(\mu_1+b)}{2P'+v(\mu_1-b)} \quad . \quad . \quad (8)$$

$$s'=\frac{M}{\mu_2}\log.\frac{2b}{(b-\mu_1)\varepsilon^{\frac{bt'}{M}}+b+\mu_1}-\frac{2P't'}{b+\mu_1} \quad (9)$$

$$s'=\frac{M}{\mu_2}\log.\frac{2P+v(\mu_1-b)}{2P'}-\frac{2P'M}{b(b+\mu_1)}\log.\frac{2P'+v(\mu_1+b)}{2P'+v(\mu_1-b)} \quad . \quad (10)$$

In this case, however, if $P'=\frac{\mu_1^2}{4\mu_2}$, then $b=o$, and the value of t' becomes $\frac{o}{o}$. By differentiation the true value in this case is found to be,

$$t'=\underset{b=o}{\frac{2Mv}{2P'+\mu_1v}}=\frac{4Mv\mu_2}{\mu_1^2+2v\mu_1\mu_2}; \quad . \quad (11)$$

and in the same case

$$s'=\underset{b=o}{\frac{M}{\mu_2}}\log.\frac{\mu_1+2v\mu_2}{\mu_1}-\frac{\mu_1}{2\mu_2}t' \quad . \quad (12)$$

If $P'>\frac{\mu_1^2}{4\mu_2}$, b becomes imaginary, and eq. (2) must be integrated differently. We have in this case, placing

$$\sqrt{4\mu_2P'-\mu_1^2}=c,$$

$$t'=\frac{2M}{c}\tan.^{-1}\frac{cv}{2P'+\mu_1v} \quad . \quad (13)$$

Therefore $s=2P'\int_0^t \frac{\tan.\frac{ct}{2M}.dt}{c-\mu_1\tan.\frac{ct}{2M}}$,

and by integration,

$$s'=\frac{M}{\mu_2}\log.\frac{c\sec.\frac{ct}{2M}}{c-\mu_1\tan.\frac{ct}{2M}}-\frac{\mu_1t}{2\mu_2}; \quad . \quad (14)$$

or introducing value of t from (13),

$$s'=\frac{M}{2\mu_2}\log.\frac{P'+\mu_1v+\mu_2v^2}{P}-\frac{M\mu_1}{c\mu_2}\tan.^{-1}\frac{cv}{2P'+\mu_1v} \quad . \quad (15)$$

Let us expand the right hand member of (13) by the formula $\tan.^{-1}y=y-\frac{1}{3}y^3+\frac{1}{5}y^5-$&c.,

$$\therefore t'=\frac{2Mv}{2P'+\mu_1v}-\frac{2Mc^2v^3}{3(2P'+\mu_1v)^3}+\frac{2Mc^4v^5}{5(2P'+\mu_1v)^5}, \text{ \&c.}$$

In this if $c=o$ all the terms of the 2nd, member disappear but the 1st, and we have eq. (11) again. Also if P' be infinite all the terms disappear; hence each term after the first must have a maximum value for some value of P'. This value of P' is easily found by differentiation, and for the second term of the series it is

$$\frac{\mu_1(3\mu_1+2\mu_2 v)}{8\mu_2},$$

and the corresponding value of the second term is

$$\frac{64M\mu_2^3v^3}{81(\mu_1^2+2\mu_1\mu_2 v)^2}.$$

This is generally a very small quantity,—in a case we shall presently discuss less than $\frac{1}{4}$ of a second,—and the succeeding terms are much smaller, so that the first term alone gives a very close approximation to the true value of t', and we may consequently use with sufficient accuracy

$$t'=\frac{2Mv}{2P'+\mu_1 v} \quad . \quad . \quad . \quad (16)$$

whenever we have $P'>\frac{\mu_1^2}{4\mu_2}$.

In Mr. Gooch's formula $\mu_2=0$; therefore we have from (3),

$$t=\frac{M}{\mu_1}\log.\frac{P}{P-\mu_1 v} \quad . \quad . \quad (17)$$

whence solving for $v\left(=\frac{ds}{dt}\right)$, and integrating

$$s=\frac{PM}{\mu_1^2}\left(\varepsilon^{\frac{\mu_1 t}{M}}-1\right)+\frac{Pt}{\mu_1} \quad . \quad . \quad . \quad (18)$$

or by (17),

$$s=\frac{Pt-Mv}{\mu_1} \quad . \quad . \quad . \quad . \quad (19)$$

Eqs. (17), (18) and (19) are for the case in which the train is accelerated. For the case of retardation, change the sign of μ_1 in (17), (18) and (19), and substitute P' for P, t' for t, and s' for s,

$$\therefore\ t'=\frac{M}{\mu_1}\log.\frac{P'+\mu_1 v}{P'} \quad . \quad . \quad (20)$$

and

$$s'=\frac{P'M}{\mu_1^2}\left(\varepsilon^{\frac{\mu_1 t'}{M}}-1\right)-\frac{P't'}{\mu_1} \quad . \quad (21)$$

or by (20),

$$s'=\frac{Mv-P't'}{\mu_1} \quad . \quad . \quad . \quad . \quad (22)$$

In Mr. Clark's formula $\mu_1=0$, hence from eqs. (3), (6), (7), (13), (14) and (15),

$$t=\frac{M}{2\sqrt{\mu_2 P}}\log.\frac{\sqrt{P}+v\sqrt{\mu_2}}{\sqrt{P}-v\sqrt{\mu_2}} \quad (23)$$

$$s=\frac{M}{2\mu_2}\log.\frac{(\varepsilon^{nt}+1)^2}{4\varepsilon^{nt}} \quad . \quad . \quad . \quad (24)$$

in which $n=\frac{2\sqrt{\mu_2 P}}{M}$

$$s=\frac{M}{2\mu_2}\log.\frac{P}{P-\mu_2 v^2} \quad . \quad . \quad . \quad (25)$$

$$t'=\frac{M}{\sqrt{\mu_2 P}}\tan.^{-1} v\sqrt{\frac{\mu_2}{P}} \quad . \quad . \quad (26)$$

$$s'=\frac{M}{\mu_2}\log.\sec.\frac{t'\sqrt{\mu_2 P}}{M} \quad . \quad (27)$$

and

$$s'=\frac{M}{2\mu_2}\log.\frac{P+\mu_2 v^2}{P} \quad . \quad . \quad (28)$$

We are now prepared to discuss a particular case. We will take a train composed of engine, tender, baggage car, and six Pullman cars.

Weight of engine=E'=70000 lbs.

Weight of train=T'=541600 lbs.

Total weight=W=611600 lbs.

$$M=\frac{W}{g}=\frac{611600}{32.2}=18993.8.$$

Let us assume that $v=60$ ft. per sec., that is nearly forty-one miles per hour.

We will first make the computation by Scott Russell's formula.

We will allow in this case, in addition to the 6 lbs. per ton for friction for the whole train, 7 lbs. per ton of the weight of the engine + 1 lb. per ton of the train exclusive of the engine for the internal friction of the same, *i.e.* $F=\frac{7T'+13E'}{2240}$

$$\therefore\ F=\frac{7\times 541600+13\times 70000}{2240}=2098.75$$

μ_1 is that part of the resistance which varies as the 1st power of the velocity taken at a velocity of 1 ft. per second, therefore from the formula

$$\mu_1=\frac{W}{3\times 2240}\cdot\frac{15}{22}=\frac{W}{9856}=62.054.$$

μ_2 is the resistance of the air at a velocity of one foot per sec.

$$\therefore\quad \mu_2=\frac{A}{400}\cdot\left(\frac{15}{22}\right)^2$$

If we assume A=100 sq. ft. we have

$$\mu_2=\tfrac{1}{4}\left(\tfrac{15}{22}\right)^2=0.1162.$$

Hence the force required to move the train at a constant velocity of 60 ft. is

$$R=F+\mu_1 v+\mu_2 v^2=2098.75+3723.22+418.32=6240.29.$$

If we should start from rest with a force $P_1=R$, we should attain a velocity of 60 ft. only after an infinite length of time, as may be seen by introducing this value of P_1 in eq. (3), first reducing it as follows:

$$t=\frac{M}{a}\log.\frac{2P-\mu_1 v+av}{2P-\mu_1 v-av}$$

$$=\frac{M}{a}\log.\frac{2P-\mu_1 v+av}{2P-\mu_1 v-av}\cdot\frac{2P-\mu_1 v+av}{2P-\mu_1 v+av}$$

$$=\frac{M}{a}\log.\frac{(2P-\mu_1 v+av)^2}{4P(P-\mu_1 v-\mu_2 v)^2}$$

$$=\frac{M}{a}\log.\frac{(2P-\mu_1 v+av)^2}{4P(P_1-F-\mu_1 v-\mu_2 v^2)}.$$

It is evident that if $P_1=R$ be substituted in this equation, we shall have

$$t=\frac{M}{a}\log.\infty=\infty.$$

Hence we must assume $P_1>R$ in order to attain the desired velocity within a finite time. Let us take it first at 7098.75 lbs., so that $P=P_1-F=5000$ lbs.; then by eq. (3), $t=471.4$ sec.$=7$ min. 51.4 sec., and by eq. (6) or (7),

$s=18084.4$ ft.$=3$ miles 2244.4 ft.

Let us call the work done by the resultant force acting on the train L_1, that actually done by the locomotive in passing over the space s L_2, and that which would be done by the locomotive in passing over the same space at a constant velocity of 60 ft. L_3, then we have:

$$L_1=\frac{Mv^2}{2}=\frac{18993.8\times 3600}{2}=34188840 \text{ ft. lbs.}$$

$$L_2=P_1 s=7098.75\times 18084.4=128377316 \text{ ft. lbs.}$$

$$L_3=R\,s=6240.29\times 18084.4=112851914 \text{ ft. lbs.}$$

$\frac{L_2}{L_1}=3.75+$, so that the work done in overcoming the inertia is only a little more than one-quarter part of the work actually performed by the engine. The difference between L_2 and L_3 represents the extra work done in getting up speed over and and above what would be done in going over the same space *at* speed. This is

$$L_2-L_3=(P_1-R)s=15525402 \text{ ft. lbs.}$$

There is a gain, however, to offset this loss, for the space passed over by the train from the point at which steam is shut off to that at which the train comes to rest is traveled without any work being done by the engine, and thus work is gained equivalent to the product of R by this distance. If we divide L_2-L_3 by R we shall obtain the distance which must be passed over in stopping the train, in order that the gain of work may just balance the loss, viz.:

$$\frac{L_2-L_3}{R}=\frac{15525402}{6240.29}=2488 \text{ ft.}$$

Hence we see that if steam is shut off at a distance of about 2500 ft. from the station there will be *no loss whatever* of mechanical work in stopping the train and getting up speed again. If the train is stopped in *less* space there will be some loss of work, if in greater there will be an actual *gain*. There will also be a small loss due to the wear and tear of brakes and the loss of time, the amount of which we shall consider hereafter. Let us next assume that the engine exerts a greater force so as to reach the velocity of sixty feet in less time, and see what the result will be.

Let $P_1=7498.75$ lbs., and, therefore,
$P=P_1-F=5400$ lbs.

Then by Eqs. (3) and (6)

$t'=392.6$ sec. $=6$ min. 32.6 sec.

$s'=14533.4$ ft. $=2$ miles 3973.4 ft.

$$\frac{L'_2-L'_3}{R}=\frac{18289672}{6240.29}=2931 \text{ ft.}$$

Thus we see that as the engine exerts more force, there is a greater loss of mechanical work, to compensate for which it would be necessary to reduce speed more gradually in stopping. It appears, then, that for economy we must "make haste slowly" in this as in other matters.

Moreover, the gain of time is but trifling, being only nineteen seconds in reaching any given point, if we suppose the train in each case to move on with a uniform velocity of sixty feet, as soon as it has attained to that speed.

We will next apply Mr. Clark's formula to the same case. Clark says that for a road badly laid, or out of repair, forty per cent. should be added to the results obtained by his formula. As his experiments were made in England, where the roads are in general better made and kept than in this country, it would seem that at least twenty per cent. should be added to obtain a fair re-

sult for the average case in this country, which would give

$$R=\frac{W}{2240}\cdot\frac{6}{5}\left(8+\frac{V^2}{180}\right).$$

Whence $F=\frac{3\,W}{700}=2621$ lbs. and

$$\mu_2=\frac{6\,W}{5\times180\times2240}\left(\frac{15}{22}\right)^2=0.84605.$$

$\therefore\ F+\mu_2v^2=R'=2621+3046=5667$ lbs.

It will be seen that this formula, even with the twenty per cent. which we have added, gives a resulting value of R considerably smaller than that of Russell. Let us first take P=3600, so that $P_1=P+F=6221$ lbs. Then from formulas (23) and (25)

$t=546.4$ sec. $=9$ min. 6.4 sec. and

$s=21004$ ft. $=3$ miles 5164 ft.

$$\therefore\frac{L''_2-L''^2}{R'}=2053 \text{ feet.}$$

In this case we have been longer in getting up speed, and find, consequently, that we can stop more quickly without loss of work.

Since we can solve equation (25) for P, we can find the force which must be exerted by the engine in order to get up speed within a given distance. For the sake of comparing the two formulæ,—those of Russell and Clark,—let us assume $s=18084$ ft. as was found in the first case considered. Then we find

$P=3806$, $\therefore P_1=6427$,

$t=483.4$ sec. $=8$ min. 3.4 sec.,

and $\frac{L'''_2-L'''^2}{R'}=2425$ ft., so that this formula gives the time twelve seconds greater, and the distance to be passed over in stopping is sixty-three feet less than that of Russell.

If we use Clark's formula without adding twenty per cent., we find R=4722.84, and when $s=18084$

$\frac{L^{IV}_2-L^{IV}_2}{R}=3434$ ft., so that as the resistance become less it is necessary to pass over more space in stopping in order to have no loss of work. We can see this at once from general considerations, for suppose that there were *no* resistances, then the work done in getting up speed would be merely that of overcoming the inertia, *i.e.*

$L_2=L_1=\frac{mv^2}{2}=34188840$ ft. lbs., while we should have $L_2=0$, R=0 and

$$\therefore\frac{L_2-L_2}{R}=\infty.$$

Thus there would *always* be a loss of work in stopping.

The results obtained by the other formulæ do not differ very essentially from those already given, though both of them give values of R at high velocities less than that of Scott Russell.

We will next take a freight train weighing 966000 lbs. in all, of which 75000 lbs. is the weight of the locomotive,

$$\therefore M=\frac{W}{g}=\frac{966000}{32.2}=30000.$$

Using Russell's formula and proceeding as before we find:

$\mu_1=98.193$ and $\mu_2=0.08135$.

Let $v=22$ ft. per sec. $=15$ miles per hour.

$\therefore\ F+\mu_1v+\mu_2v^2=3219.6+2160.2+39.4=5419.2$.

Suppose that P=2800, $\therefore P_1=6019.6$; then by equations (3) and (7)

$t=460$ sec. $=7$ min. 40 sec.

$s=6343$ ft. $=1$ mile 1063 ft.

$\therefore\ L^{V}_2=38181114$ ft. lbs., $L^{V}_2=34373032$

and $\frac{L^{V}_2-L^{V}_2}{R}=\frac{3808082}{5419.2}=703$ ft.; so that in this case it is only necessary to shut off steam at about 700 ft. from the station in order to avoid loss of work.

We will now consider the relations of force, time and space in bringing the train to rest. Taking the express train first treated, with a velocity sixty feet per second, we find by Russell's formula when

P=F=2098.75 lbs., $t=305$ sec., and $s=7524$ ft.,

P=3000 lbs., $t=242$ sec., and $s=6220$ ft.,

P=4000 " $t=198$ " and $s=5239$ ft.,

P=5000 " $t=168$ " and $s=4529$ ft.,

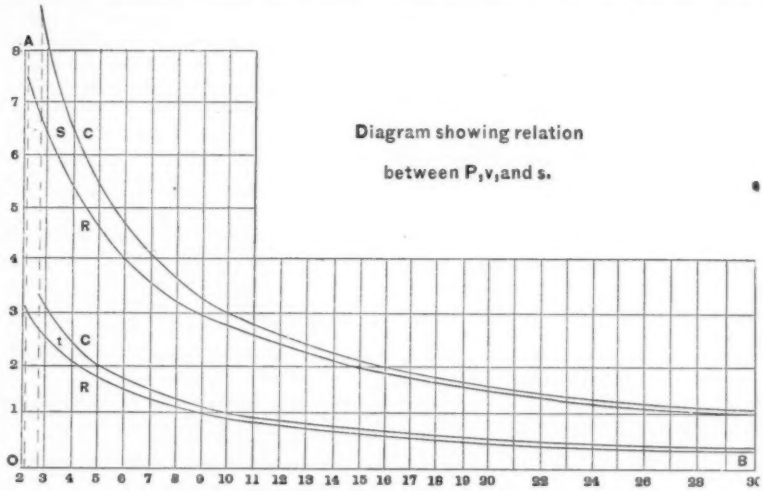
P=6000 " $t=146$ " and $s=3991$ ft.,

$P=\frac{\mu_1^2}{4\mu_2}=8284.5$ lbs., $t=112$ sec., and $s=3143$

P=10000 lbs., t=96 sec., and s=2701 ft..
P=15000 lbs., t=67 sec., and s=1939 ft.,
P=20000 lbs., t=52 sec., and s=1489 ft.,
P=30000 lbs., t=36 sec.,
and s=1000 ft. nearly.

Thus according to this formula the train would come to rest by the operation of ordinary resistances without using the brakes in about five minutes, passing over in that time a distance of about 7500 feet. Since Clark's formula makes the resistances somewhat less, it follows that it would show a greater brake-force to be required to stop the train in a given time or distance.

According to this formula, when
P=F=2621 lb, t=332 sec., and s=8655 ft.
P=3600 lbs., t=256 sec., and s=6882 ft.,
P=4900 lbs., t=197 sec., and s=5426 ft.,
P=6400 lbs., t=156 sec., and s=4370 ft.,
P=8100 lbs., t=126 sec., and s=3583 ft.,
P=10000 lbs., t=104 sec., and s=2984 ft.,
P=20000 lbs., t=57 sec., and s=1591 ft.,
P=30000 lbs., t=38 sec., and s=1085 ft.



The accompanying diagram will show clearly the relations between these results. The numbers along the horizontal line O B give values of P in thousands of lbs., those along the vertical O A give t in hundreds of seconds, and s in thousands of feet, assuming that v=60.

The lower pair of curves, marked t, are time curves, the upper of the two, marked C, being constructed by Clark's formula, and the lower, marked R, by Russell's. The upper pair, marked s, are space curves, the upper of the two as before being found by Clark's formula, and the lower by Russell's. Thus if it be desired to find P and t corresponding to s=2500, draw a parallel to O B half way between 2 and 3 on the line O A till it cuts the two space curves, then drop perpendiculars from these points of cutting on OB, cutting the time curves. These ordinates will give the time by the two formulæ, and the abscissas on O B will give the corresponding values of P. Thus we find t_R=90 sec., P_R=11160, t_c=91 sec., and P_c = 12370.

It will be seen by looking at eqs. (23) to (28), and remembering that by Clark's formula μ_2 depends on W and therefore on M, that no effect will be produced on the values of s and t by changing W, provided that P is changed in the same ratio, and v is unchanged. The same holds only *approximately* for Russell's formula, since in this μ_2 is independent of W. Thus we can find the relation between P, t and s in stopping a train of any weight moving with a velocity of 60 feet by these same curves, and if a series of such curves were constructed for different values of v we should have a diagram applicable to any case. Similar diagrams might also be made for the case in which the train is getting up speed. Now let us see what time is lost in stopping. Taking the same express train we have generally considered, suppose that it stops in 2500 feet, and in 91 sec., as just found, and that it gets up

the velocity $v=60$ in 18084 feet, and in 483 sec. as found by Clark's formula; then the entire space passed over in starting and stopping is 20584 ft., and the time occupied is 574 sec. Now the time occupied in passing over the same distance at 60 ft. per second is $\frac{20584}{60}$ $=343$ sec., so that the loss of time $=574-343=231$ sec. $=3$ min., 51 sec., to which must be added the time during which the train is actually at rest, varying usually from 0 up to 20 minutes.

Thus it appears that the loss of time in the actual processes of stopping and starting is comparatively small.

Let us now take up the subject in a somewhat different way. Suppose we have two stations 30 miles $=158400$ ft. apart, and a train is required to go from one to the other in a given time, say 48 minutes, let us see what will be added to the expense if we introduce a way station, still requiring the whole distance to be made in the same time. We will assume that the train gets up speed in 20000 ft., at which point the steam pressure is so reduced as to keep the speed constant, and that 2500 feet, are passed over in stopping.

Suppose that when there is no intermediate station the rate of uniform speed is $v=60$ ft. per sec. Then by eq. (23) the time of getting up speed is 525 sec., and by (26) that of stopping is 91 sec. The distance passed over at a uniform rate is:

$158400-20000-2500=135900$, and the time is $\frac{135900}{60}=2265$ sec.; therefore the whole time is $2265+525+91=2881$ sec. $=48$ min. 1 sec. The value of P while the speed is increasing is 3662.3 lbs., and therefore that of $P_1=P+F=6283.3$ lbs. While the velocity is uniform $P_1=R=5667$.

In the first case the work $=L_1=20000\times 6283.3=125666000$ ft. lbs.

In the second case the work $=L_2=5667\times 135900=770145300$ ft. lbs.

$L_1+L_2=L=895811300$ ft. lbs.

Now if a station be made at some intermediate point a greater velocity must be given to the train when it is moving uniformly. We will suppose that the train just comes to rest and then immediately starts on again. We can arrive at the value of v by a series of trials, a first approximation being obtained by supposing the same time to be occupied in stopping and starting with the new value of v as with the old. Thus if we subtract from the whole time, *i.e.* 2881 sec., twice the time of starting and stopping, or $2(525+91)$, the remainder is the time during which the train moves uniformly. The corresponding space is $158400-2(20000+2500)$, and if the latter be divided by the former we have $v'=68.7$ ft. This is evidently too large, because the average velocity of stopping and starting being greater than before, the *times* will be less. By trying successively several numbers smaller that 68.7 we find that $v'=65$ ft. is very nearly the correct value. For corresponding to $v'=65$, we have time of starting $=485$ sec., time of stopping $=80$ sec., time of moving at uniform rate

$$=\frac{158400-2(20000+2500)}{65}=1745 \text{ sec.}$$

$\therefore$ the whole time $=1745+2(485+80)=2875$ sec., which differs only 6 sec from the required time, and shows that 65 is a very little too large. It is near enough, however, to the true value for our purposes. We find also corresponding to $v'=65$, $P'=4298.1$, and therefore $P_1'=6919.1$, and $F+\mu_2 v'^2 R'=\;=2621+3574.56=6195.56$. Hence

$L_1'=6919.1\times 2\times 20000=276764000$ ft.lb.

$L_2'=6195.56\times 113400=702576504$ ft. lbs.

$\therefore$ $L_1'+L_2'=L'=979340504$ ft. lbs., and $L'-L=83529204$ ft. lbs. is the work expended over and above what was expended when there was no intermediate station. To see what $L'-L$ means, we will find how far it would carry the train at a uniform speed of 60 ft. The distance is

$$\frac{L'-L}{R}=\frac{83529204}{5667}=14739 \text{ ft.}$$

a little over $2\frac{3}{4}$ miles. Thus the extra work required on account of the inserted station, would be sufficient to carry the train over $2\frac{3}{4}$ miles at its ordinary rate, and the question to be decided would be whether the gain of time, amounting to 247 sec. or 4 min. 7 sec. would be of sufficient value to offset the loss of work. The disadvantage of the way station would be shown in a yet stronger light if a stop of several minutes were made there, and the whole distance were still traveled in the same time.

Most of the results of this article have referred to a passenger train whose total mass was taken as 18993.8, g being assumed at 32.2. If the value of g be different from 32.2 the only effect will be to cause the results to refer to a train whose *weight* is a little greater or less as the case may be.

THE GROWTH AND PRESENT POSITION OF THE SCIENCE OF MACHINES.

By Professor A. B. W. KENNEDY, C.E.

From "Journal of the Society of Arts."

I have to-night to endeavor to give you a sketch of the development of a science, or branch of science, in which the members of the Society of Arts may be supposed to have some special interest—the science of machinery. Like other sciences, this one began to grow long before it existed. Anything like a complete scientific treatment of machines—their form and objects and dimensions—is of very recent date, and even the first faint traces of such a treatment do not date back more than a century and a half. Without going back to Hero or Archimedes, however—to say nothing of Aristotle—I must start at a somewhat earlier period than that I have named, if I am to give you, as I hope to be able to do, any connected account of the ways in which machines have been viewed and treated by the men most capable of thinking about them, from the time when they began to put their ideas into writing—from the time, that is, at at which their ideas first become known to us. We shall find that it is hardly possible to say exactly when these ideas first took anything like a scientific form, and it is not a question in which one can feel any great interest. It is more important and more instructive to notice that we can trace continuous, uninterrupted development, step by step, from Ramelli and Leising right on to Tredgold, Willis and Rankine, Monge and Hâchette, Redtenbacher, and Leuner, and Reuleaux. It is the course of this development which I wish now to indicate to you.

At the outset, let me just remind you that it is not the growth of the machines themselves about which I have to speak, but the growth of their theory. Many names that are familiar in connection with the former have little place in regard to the latter, as you will find, and *vice versa*. Nor have I this evening to do directly with the growth of the science of mechanics, which I find had, by a little oversight, been announced as my subject. The general course of the connection between mechanics and machine science has been something like this: At the very beginning mechanics was certainly identical with the science of machines, but although this was the case—although, that is, the science of mechanics contained all that was scientifically known about machines—yet we shall find that the two were in reality very far separated. The engineer made his machines with scarcely any knowledge of mechanics, the mathematician evolved his mechanics with very little reference to machines. In time, the nominal connection between them grew weaker, as the science of mechanics received higher and more general treatment, while, again, later still, we find certain portions of that science specially connected with engineering treated by themselves as "practical mechanics," the more general treatment of the subject for its own sake being called "theoretical mechanics." By this time, however, machine science, or some part of it, had begun to have separate existence, and stood beside, rather than coincided with mechanics.

With this much of introduction, I may enter at once into my subject.

The literature of machinery dates from the latter part of the sixteenth century, and then it certainly makes an excellent start. Within a few years of each other appeared the "Mechanicorum Liber," of Guido Ubaldus (1577); the "Theatrum Instrumentorum," of Besson (1582); and the "Arteficiose Machine," of Ramelli (1588), each of them a substantial folio, such as a modern publisher would scarcely venture to bring out, and a modern public would hardly purchase very largely.

Let me indicate to you, as briefly as I can, the view of their subject taken by the writers I have named, and others of the same epoch. Its most noticeable characteristics is perhaps the absolute separation between mechanics and machinery which I have already mentioned. In the time of Archimedes, the mathe-

matician was the engineer; the student of mechanics was the inventor of machines. During the centuries that had passed over the world since then, their relative position had altogether changed. The bulky "Mechanicorum Liber"—which consists of treatises on the balance, the lever, the pulley, the wheel and axle, the wedge and the screw—contains little that had not been given by Greek mathematicians centuries before. The problems are much the same, the point of view from which they are looked at is much the same, the solutions are much the same. Nominally, they have to do with physical data and phenomena; actually, they are no more than geometric exercises worked out in the study. But while the science of mechanics was thus in its infancy, gradually becoming a not unimportant branch of mathematics, the class of men to whom in time it was to be of the most direct value—the forerunners of our present engineers—had found themselves face to face with a very different set of problems. They had had to make pumps and water-wheels, mills, apparatus for land and water transport, hoisting apparatus, bridges and roads and viaducts, and so on. Every one of these brought them into direct contact with nature. Their difficulties were not connected with lines and circles, and mathematics could help, as yet, very little in solving them. They had to deal with some of the most complex problems in physics—problems which, even now, we have not completely mastered; and physicial science was as yet unborn. It must be admitted that the cut-and-dried lever and wedge and wheel of the mathematician, and his neat geometric figures cannot have appeared to the engineer, struggling mightily to utilize and control forces which he could not see and energies which he could not understand, to have much direct bearing on his work. We cannot wonder that, when at length he became articulate, his utterances took a form very different from those of the writers on mechanics.

The "Mechanicorum Liber," as I have said, is a collection of treatises on the mechanical powers; that unfortunate jumble of apparatus which has played the part of the "old man of the sea" to machine science, and which we have scarcely yet got altogether thrown from our shoulders. It is purely theoretic, and its theory is, in general, sound enough and useful, of course, now that we know how to apply it. At the time, it was no doubt regarded as a mathematical book, the "simple machines" only illustrating mathematical problems, and not as setting forth in any way the principles upon which machine design was based.

The "Diverse et Arteficiose Machine" of Ramelli, and the "Theatrum Instrumentorum" of Besson, are much more interesting to us. The authors were both men of distinction in their time, Captain Augustus Ramelli being an engineer in the service of the "most Christian" King of France, to whom he dedicates his book, and Besson, a doctor of mathematics.

Their books, therefore, which are very similar in form, will enable us to form some idea of the way in which the best engineers of that time looked at their work. Ramelli's, I may mention, is given, chapter by chapter, in both French and Italian; Besson's is in Latin, but a French translation of it was published in 1596. They consist solely and simply of descriptions of machines. There is absolutely no referenee to general principles, not even to the mechanical powers. One plate (the engravings in the "Arteficiose Machine" are extremely beautiful) follows another, and for each there is a separate description. A connection is recognized between machines having the same general purpose, but this purposive connection seems to be the only relation distinctly realized by the authors. Ramelli, *e.g.*, begins his book with a hundred and thirteen machines for raising water. Each one has a plate and a description to itself, and each description begins in the same way: "C'este cy est un autre façon de machine par laquelle." . . . . "Une autre façon de machine par laquelle." . . . . "L'effet de ceste autre façon de machine est de faire . . . monter l'eau," and so forth, and goes on to detail the whole construction *de novo* from the suction-pipe to the discharge-nozzle. In this way, nearly every machine is described many times over; each one comes first worked by a water-wheel, and then by a man or an animal. Then the same machine reappears, with

such minute alterations as the substitution of a figure of Neptune for the pipe through which the water is discharged, &c. But each time the whole description is repeated. Further on, come other machines, possible and impossible, treated in just the same way. A screw lifting-jack, for instance, is a machine for lifting a prison door off its hinges, and after being described separately once, is several times re-described in this particular application. A wrench is a machine for breaking the bars of a grating, and so on.

The authors of these early *theatri* view each machine, as a whole, only as one piece of apparatus, intended to do one particular piece of work. They recognize that, as a fact, it is made up of a number of different pieces; but, as soon as any one piece has become part of the machine, it seems to lose its identity; it no longer has any separate existence. An exactly similar piece in the next machine has to be again described, and so on in the next, and the next. Everything, moreover, as I have said, is looked at purposively in the most specialized fashion. The general ideas of, and of course the corresponding words for, pump, crane, hoist, wrench, and so on, are wanting. One is a machine to lift water from a well by means of a man; another to lift water from a river by means of a man; another to lift water from a well by means of a water-wheel; one to raise water for a fountain by means of a water-wheel; another by which a man can raise water for irrigation; and so on with the other classes of machines.

Then, further, there is no critical examination of the machine whatever; not even a statement that one is better than another, or simpler or more efficient. There is nothing but a statement of the machine's purpose, and a description of its construction. Ramelli's descriptions are very detailed, his plates being carefully lettered throughout. Besson's descriptions, on the other hand, are not nearly so minute, for the reason, among others, that his system of references is not a very convenient one. To avoid putting letters on his plates, he uses special names for top and bottom, right and left, calling them septentrionale, meridionale, orientale, and occidentale (these words being printed on every plate), and also special names for the corners, and then indicates, by the help of these, the portion of the plate in which any particular detail is to be found.

It must, of course, be remembered that the machines described in the *theatri* had no actual existence in many cases. Which did exist, and which did not, is, of course, now impossible to say, but certainly many of them are physical impossibilities (the lazy-tong bridge and the ship-lifting lever, for example). Ramelli's machines are drawn in such minute detail that I cannot help believing that he actually made models of them, from which his drawings were made. Besson, on the other hand, while far less minute in his details, contains fewer machines which are obviously mere inventions never practically executed.

The drawings in all the works of this time are, of course, in perspective, not arranged in plan and elevation—an improvement which was not long in coming.

I have endeavored to give you some notion of the light in which the machine appeared to the very earliest authors who gave it a place in literature, at a time when as yet machine science did not exist. We have now to trace the growth of this special branch of literature and the development of the science to which it gave rise.

The attempt to connect mechanics with machinery was very soon made. In 1608, the first volume of Leising's "Theatrum" was published, in which the editor, mourning over the lamentable ignorance of practical men, incorporated a translation of a short Latin treatise, lately published, on "The Proper Foundation and Understanding of the Balance and Weight," by "that learned and honorable physician and mathematician, D. Eualterus Herminius Rivius," to which he added some general chapters on the meaning of the word machine, the scope of machine study, the origin of machines, the first inventors, and so on. But, as was to be expected, the introductory treatise, which is to give all readers full instruction for the understanding of all the machines, has, after all, very little to do with what follows. The author soon gets tired of his high

horse, and descends to very perfunctory descriptions of his plates, which are to a great extent inferior reproductions of those given in the earlier and larger *theatri*.

The work is notable, chiefly on account of the distinct recognition in it, by an engineer, of the fact that his art had also a science. For, although many former works on mechanics had professedly treated of the science of machinery, it is obvious that, to the actual engineer—the veritable constructor of pumps and water-wheels, and other such "engines"—no such science existed. He worked away at his clumsy, but useful contrivances, without saying—or, at least, without writing—much about them; but the lever and the balance, in their mathematic form, were wonders to him, mysteries—things hard to be understood, but greatly to be respected. He did not recognize at first that they had much connection with his work, although he was continually making use of them. And this is not to be wondered at, for the theorists delighted chiefly in describing the enormous loads which could be raised by them, and the smallness of the effort required for that purpose; their levers would do anything—on paper. But he, poor fellow, found that the nearest approach he could make to the mechanician's lever was a ponderous, unwieldly wooden bar. It certainly gave him a very distinct "mechanical advantage;" but then, the effort required to move his load was something out of all comparison with the breath, or the touch of a straw, or pull of a thread, which ought, with a real lever, to do such wonders. And, worst of all, his lever required a very considerable effort to move itself, and that very few of the paper levers seemed to do. Thus, altogether, we cannot be astonished that he failed to discover at first any definite connection between his own machines, which were so hard to move, and these wonder-working contrivances, that did so much with so little trouble. They belonged to distinct *genera*. Herr Leising is not moved to astonishment over his pumps and bridges, and lifting-jacks; it is only of the "lever" that he tells us, very philosophically, that, "when we consider the matter, we shall find that those things which perform wonderful actions must spring from wonderful causes." "Is it not extraordinary," he says, "that an extremely heavy load can be easily raised by adding another weight to it?" and proceeds to discourse on the wonderful qualities of the circle, "the most beautiful, complete, and strong of all figures," and, following Aristotle, traces to these all the mechanical wonders of machines.

The timid attempt of Leising to combine mechanics and engineering was soon followed by bolder steps in the same direction. The work of Solomon de Caus, "Des forces Mouvantes," was published in 1615. It differs from its predecessors in several points. There is a distinct recognition of the connection of machinery with physicial science generally, and there is really some attempt to place machine problems on a scientific basis. The attempt breaks down altogether, as did most other such attempts for more than a century after the time of Caus; but it is not with its success, but with its existence that we have here to do.

His book commences with a series of physical definitions and theorems, and then, after examining the "mechanical powers," he goes on to "problems," which are, for the most part, such as require for their solution simply the description of a machine, old or new. Between the theorems and the problems there is no direct connection, but a stage had been reached at which it was recognized that engineering problems were based on physical and mathematical science; and that was a great deal. But the sciences had to advance much further before they could be actually applied to the complex problems of engineering, so that at present they could simply be put side by side. It was reserved for after generations to show the real nature of the connection between them.

Caus does, however, make two distinct and important steps forward. First of all, in his plates he shows the essential part of a machine, sometimes apart from their ornamental surroundings. The head of Bacchus, through whose mouth water is pumped, is no longer as necessary to the pump as the piston itself. And secondly, he gives what may be called "working drawings" of their de-

tails, in the conventional representation now universally used, and called orthographic projection. He calls such a drawing an "orthographie." Previously all drawings of machines had been given in perspective. All engineers will recognize the importance of this change. In this book, then, which is before you, is perhaps the first published working drawing, or scale drawing, of any machine.

I am sorry to say, that our own country did not do much at this time towards the development of machine science, whatever may have been done here in improving machines themselves. The first work in our own language upon mechanics, and almost the first work on the subject published in England, bears on the title page the inscription, "Mathematicall Magick, or the Wonders that may be performed by Mechanicall Geometry. In 2 Books. Concerning Mechanicall { powers. / motions. } Being one of the most easie, pleasant, useful (and yet most neglected) part of Mathematicks, not before treated of in this language. By J. W., M.A." The author was no other than John Wilkins, afterwards Bishop of Chester, at that time chaplain to the Prince Elector Palatine. He has certainly succeeded in producing a very entertaining book, but it cannot be said that he did anything to advance the science of which he was so much enamored. I fear, indeed, that his book had an opposite effect, if it had any effect at all. The Bishop seems to have been chiefly attracted by those parts of his subject which savored of the marvellous, in this as in his other writings. He gives us a drawing, for instance, of a train of wheelwork and pulleys, by means of which a puff of breath, or a pull by a hair, or a push with a straw, could uproot an oak-tree, or, "a power, which is much less than the one-hundredth part of a pound will bee able to move the world;" and he gives us all his calculations to show it. But, as usual, the poor engineer fares but badly at his hands, for he goes on, "It were needlesse to set down any particular explication how such mechanicall strength may be applyed unto all kinds of local motion; since this, in itself, is so facill and obvious that every ordinary artificer doth sufficiently understand it." Wilkins was very credulous as to everlasting lamps, and subaqueous traveling, flying chariots, and so on. In speaking of flying, he tells us that "the wings of a bat are most easily imitable; perhaps nature did by them purposely intend some intimation to direct us in such experiments, that creature being not properly a bird, because not amongst the *ovipara*, to imply that other kind of creatures are capable of flying as well as birds, and if any should attempt it that would be the best pattern for imitation." One good service Wilkins certainly did, however. Although most amusingly anxious to discover and believe in a perpetual motion, he examines a number of proposed arrangements for that purpose, and points out that all of them are based on misconceptions, and that all have failed on trial.

After the time of Caus no very considerable progress seems to have been made until near the close of the century. The "Nouvelle Invention de lever l'eau," of Isaac de Caus, engineer and architect to Charles I., published in London (in French), in 1657, does not show much difference from the work of the elder Caus, from whose book most of his engravings are copied. During this period appeared also Moxon's "Mechanick Exercises; or, the Doctrine of Handyworks" (1677, and intended to be continued monthly). It is a detailed description of the construction of lathes, tools, locks, &c., and of technical processes and methods, and was thus entirely untheoretical. No doubt the book was a valuable one in its own range and period, although it does not concern us here.

Towards the end of the seventeenth century we can see distinct marks of progress, made, no doubt, slowly, but becoming visible to us more or less suddenly. Of these, the two most important are the use of geometrical construction in the solution of mechanical problems, and the consideration of the strength, or molecular resistance, of the materials from which the machines are formed. Of the first of these I shall have to speak further on. I shall only say here that the earliest work on mechanics in which I find constructions systematically used, in place of elementary algebraic proofs or arithmetic illustrations, is the "Traité de Mécanique" of

the mathematician De La Hire (1695), which, on this account, marks certainly a notable epoch. It is worth noticing that De La Hire, in resolving forces, uses a triangle, and not a parallelogram. The overpowering influence of Newton, coming but a few years later, has made the use of the parallelogram almost universal, and it is only within the last few years that we have returned to the simpler figure. Even now, in books from whose authority one would not expect it, we find that conservative instincts have overmastered reason, and the student is told to draw five lines in order to see half the solution of a problem which can be perfectly solved by three.

De la Hire's work is also remarkable as being one of the earliest in which the resistance of materials to fracture was treated of. This subject, the "strength of material," which now forms the physical basis on which a large branch of machine science—namely, machine design—rests, was first worked in by Galileo, who published a theory of beams. Mariotte had, at this time, also written on the same subject. De La Hire introduces it as an essential part of mechanics, which meant, with him, the science of machines. In his chapter on the subject he adopts the theory of Galileo, which was an erroneous one, but which, nevertheless, in certain special cases, gave results identical in form with those now known to be true. In the absence of experiments to determine the value of constants, of course the theory, right or wrong, did not admit of any direct application in machine design. Another century had passed before we find traces of rational proportioning, by engineers, of the dimensions of the rods, or links, or beams of their machines to the loads or pressures they had to sustain.

Some practical work in machine design, however, De La Hire did, for he gave constructions for determining the forces of cams, and of the teeth of spur wheels, which might have been, and no doubt were, used in practice. If this were the case, we have here the first definite point of contact between the theoretical mechanics of the time, and the actual work of machine design or construction.

In passing, I may mention, that just at this time, viz., in 1696, the second English work on mechanics was published. Its title gives a good idea of its general drift, "Mechanick Powers: or the Mistery of Nature and Art Unvailed; showing what great things may be performed by Mechanick Engines in removing and raising vast weights with little strength or force; . . . . . a work pleasant and profitable for all sorts of men, from the highest to the lowest degree, and never treated of in English but once before, and that but briefly." Its authors were Venterus Mandey and James Moxon. While the work of these worthies was doubtless a useful one to many readers, it does not in itself do anything to advance machine science.

The chief article of faith of writers on machine-mechanics up to this time, and indeeed for long afterwards, was, that certain contrivances called "mechanical powers," or "simple machines," were the elements from which all other machines, "compound engines," as they were called, were built up. There can be few things more remarkable in the history of science than the way in which this statement has been repeated and repeated, for centuries, without the least hint that it was not all right, by men who would have seen, if they had but honestly thought out the matter for themselves for five minutes, that it was not true. It is a somewhat striking commentary on the value of the historic argument for the truth of an opinion or hypothesis. The statement was first made by those who, with very limited knowledge and experience, really believed it to be true; it was repeated over and over again by others of greater knowledge and experience, simply because they would not take the trouble to think for themselves, to throw off the overpowering pressure of an ancient and venerable tradition.

The matter becomes in this case the more remarkable because no two writers agree as to which combinations are to be called the "mechanical powers," nor how they are related to each other, nor indeed of what each one essentially consists. Still less does anyone succeed in showing, either by analysis or synthesis, how the compound machines are built up from the simple ones. This very vagueness, no doubt, added greatly to the ease with which they might be dogmatized

about. But no sham can exist without doing evil, and here the evil has been lasting and serious. Attempts to apply mechanics to machinery began naturally enough by applications of mechanics to the mechanical powers, the simple machines. But their beginning was their end. The connection between the simple and compound machines was so obvious that it went without proof, while at the same time it was so subtle that all attempts to get hold of it were failures. And so for two centuries we have over and over again the same story. The writer of a treatise on machine-mechanics gives us drawings of impossible levers, improbable screws, and unheard of wedges, and duly repeats, with more or less of variation, the received theory of these instruments, and here he stops. I fear the average engineer did not derive much benefit from his well-intentioned instructor. His lever was not a bent rod resting upon a triangle like the lever in the books, his screw never would do the wonders that it ought to have done, and his "compound engines" did not, in general, contain wedges or "funicular machines." We recognize that there may have been some good ground at one time for the now traditional distrust of "practical" men for what they call "theory." Certainly at one time the theory may have been called unpractical in the fullest sense of the term.

From the beginning of the eighteenth century the interest in the "marvellous" characteristics and properties of machines diminishes, and they are treated much more as familiar things. A healthy spirit of scepticism, moreover, respecting the prodigies to be performed by the mechanical powers, began to spring up. Thus Sturm, in his "Mathesis Juvenilis" (of which a translation was published in London in 1708, and which was a very great advance on Mandey and Moxon), devotes a good deal of space to the demolishment of Archimedes and the world-moving lever with which tradition credits him. After elaborately estimating the probable weight of the earth, he calculates through how many thousand million miles Archimedes would have had to move his hand in order to move the earth the thousandth of an inch, assuming most exactly that the earth rested on his lever at one foot from the fulcrum, and that he could exact a pull of a hundred pounds, and calculates also how many hundred million years the operation would have occupied. From this very matter of fact point of view, he comes to the general conclusion that a great deal of the "tall talk" which had been indulged in as to the power of the lever and so on, was nothing more than talk, and was, therefore, much to be deprecated. He was one of the earliest to realise clearly a point that had been known well enough before, viz., that (to use his own words) "the immense force of the mechanical powers . . . . . would be infinite and altogether stupendous if it were not that the greater their force is the more slow their operations are." His services in this respect deserve special mention, for the matter was one which had led to the writing of much nonsense. The worthy Bishop Wilkins, for instance, although he knew the fact perfectly well, and even wrote a chapter about it, yet seemed to realise it so imperfectly, or else was so enamored with anything savoring of the marvelous, that his knowledge of it does not for a moment prevent him from giving a detailed description of his tree-raising machine and others of the same sort, or from such statements as these:—"By these mechanical contrivances, it were easier to have made one of Sampson's hairs that was shaved off, to have been of more strength than all of them when they were on."

Having mentioned Sturm (who was a professor at Altorf), I may further use his work as an illustration of the utter helplessness of the system of mechanics, based upon the "simple machines," in the presence of the more complex instruments of every-day life. In the "Mathesis Juvenilis" is a chapter headed, "Compound engines and their forces, which may immensely be increased." Here was certainly the place to say something about the important machines used in the arts, with at least the existence of which he must have been perfectly familiar. But it is obvious that he did not really believe these machines to be formed from his elements, for he can only mention two "compound engines," of which one was the lifting tack, and the other a machine like that "probably" used by Archimedes (the "Charistion")

for drawing his enemies' ships out of the water. Engineers looking to the book for help in their work, had to content themselves with this, and the cheering intimation, than besides these two "compound engines," "there are innumerable ones daily invented, as also several that have been actually made and tried by experience!"

The well-known "Theatrum Machinarum," of Leupold (1724), marks an epoch in the literature of machinery. It is an immense work, extending to nine folio volumes, and containing descriptions of all the principal classes of machines used in the arts. In it we can mark most distinctly the transition from the old to the new standpoint, from wonderment to criticism. (Two earlier works cited with favor by Leupold, Mögling, and Jungnickel, no doubt led up to him, but these I have not been able to find in London.) Leupold was a competent mathematician as well as a practical engineer, and his critical remarks are often sound and acute enough, taking into account the state of contemporary science and practice. The most notable feature about the work is one which the author does not remark or appear conscious of. To Leupold the machine is no longer a whole—as to Ramelli and Leising and most of the earlier writers—but a combination of parts, each of which may be isolated and examined separately. Leupold does not state this anywhere, in as many words, he simply takes it for granted—no doubt in this merely carrying on one step further a process which had been developing itself for a century past. After his definition of a machine, which runs:—"A machine or engine is an artifice by which some advantageous motion can be obtained, and something moved with a saving of time or force which would not otherwise be possible," comes the stereotyped "Machines are either simple or compound. The simple machines are the so-called mechanical powers," viz., . . . and so on. . . . . "Compound machines consist of two or more similar or different simple ones." But like every other writer, he no sooner makes the statement than he throws it aside and uses it no more. He goes on to treat in separate chapters, and in similar ways, not only the mechanical powers but also cranks, fly-wheels, pendulums, cams (oval discs, he calls them), lazy-tongs, weights and springs. In these chapters, and throughout his book, it is evident that he looks at the machine essentially as a combination of mechanisms, the first great step in machine analysis. Leupold also treats systematically the external moving forces or agents, and the means employed for most conveniently utilising them; cranks and wheels for human and animal power, the sails of wind-mills and the wheels of water-mills. He has a chapter also on the "force of fire," but of this, perhaps, the less said the better, for at the time he published his theatrum, his idea of utilising the said force in a fire-wheel was not a very satisfactory one. He had not at the time invented his high-pressure engine.

It is evident from the plates in Leupold's volumes and those of his contemporaries, that a very great improvement had been gradually taking place in the accuracy of construction of machines, *i. e.*, the accuracy with which their different parts were made to move in the required paths. An engineer of Ramelli's time can scarcely have recognised that each of his rods and links should have some definite motion, for certainly (judging from his drawings) they must have had very indefinite motions indeed. The constraint of the motion had been obtained and its necessity tacitly recognised at the same time. The improvement in the construction of machines pointed out the direction for the development of their theory, and so far as Leupold's work was scientific, it was just this part of the science that he took up. His chapters on mechanics are very elementary, and contain scarcely anything that had not been given many times before, and his detailed work is mostly descriptive rather than scientific. He does, however, distinctly apply geometrical methods to the determination of the forms of different parts of machines, and to the investigation of their motions; and here we have the beginning of machine-kinematics, the first appearance of any science of machines separate from mechanics.

From this time the writers and books on machinery rapidly increase in number, and we can trace the development—or, if you like, the formation—of machine

science along three distinct lines. Of these, one has the direction I have just indicated, and may be summed up, in one word, as the geometry of machinery. It connected itself with the motions—considered merely as changes of position—of the various bodies forming the machine. It included both the study of these motions in themselves and in their relations to one another, and the study of the geometric form of the bodies by which particular motions could be obtained. I think there can be no doubt that the increasingly great necessity for such investigations, and the necessity for carrying them on by grapho-geometric methods, in order to be of use to practical men for technical purposes, must have had an important influence in the bringing back of the study of pure geometry, which had for so long been neglected by mathematicians. In any case, the reaction in favor of geometry at the end of the last century soon had its effect on machine science. At the foundation of the Ecole Normale in Paris, in 1794, Monge introduced there his descriptive geometry, and his colleagues and successors—Hâchette, Lanz, Betancourt, Borgnis, and others—systematised machine-kinematics, and brought it into the form in which, essentially, it has until recently remained.

The application of geometry to machines formed one line of advance. A second was formed by the applications of physics, especially in connection with hydraulics, and, later on, with heat—it would hardly be correct to speak of thermo-dynamics at that date. These subjects were, of course, connected chiefly with the theory of prime movers, and the former was, comparatively speaking, of far greater importance then than now, for, at that time, water-wheels were the most important prime movers, and the raising of water was, perhaps, the most important application of machines. Among the names most closely connected with the progress of machine science in this direction, are those of Belidor, Prony, and Langsdorf. Belidor's "Arch. Hydraulique" (1782) was or its time an extremely valuable work; it is a treatise, theoretical and descriptive, on every possible machine or structure which has anything to do with water. In Prony's "Nouvelle Architecture Hydraulique" there is far less description and more theory; and in Langsdorf's "Lehrbuch der Hydraulik" but little of the descriptive matter remains. His pages bristle with immense formulæ, and long explanations of the symbols used in them. Considering the great uncertainty of many of his data, and the want even now of trustworthy experiments on the subject of which he treats, I cannot imagine that his closely-printed volume was of much direct use to any engineers in their practical work. None the less, his labor was well spent in showing that the subject was one which could be treated scientifically, and in which results could be obtained by other than the mere "rough guess and divide by two" method, which had been till then almost universal.

I have mentioned De la Hire's work in connection with the resistance of beams; it was merely theoretical, without numerical values of any constants, and interesting, therefore merely historical. Since his time several great mathematicians had written on the subject, and notably Bernouli had pointed out that Galileo's theory of beams was incorrect, and that there existed in a beam a neutral axis, on the two sides of which the senses of the stresses was opposite. He had not continued to work at the subject, however, and so little practical interest was taken in it, that for many years no engineer seems to have even alluded to it, or thought it worth while to work at it. It is only towards the beginning of the present century that we find definite progress made in this direction in applying the laws of the strength of materials to the design of machinery.

Even in such works as those of Prony and Langsdorf, already cited, no mention is made of the possibility of proportioning the sizes of the pieces of a machine to the loads they have to bear, or the pressures they have to resist; the only exception I have found is a chapter by Langsdorf on the thickness of water-pipes. This can hardly have been owing to any inherent difficulty in the subject, for the author did not scruple to attack the much more complex problems of hydraulics unflinchingly. No doubt it was due chiefly to the lack of experimental data, for without any means of determining the values of constants, it

must have seemed a somewhat useless task to write down formulæ. The earlier experiments of Müschenbröck and others had been conducted with woods of different kinds, and had given results so discrepant as to be of little practical value. Iron was now more and more superseding wood as a constructive material; but as yet estimations of its strength were little more than guesswork, and, indeed, for some time after determinations of the tenacity of iron had been made, you find it given in books only as so many times stronger than pine or oak as the case may be.

I have not mentioned our own country in speaking of the progress made during the last century, and for the good reason that our engineers had been busying themselves about other matters, and had aided the progress only indirectly. If my subject were machines themselves, and not their theory, I should, as you know, have much to say about the illustrious engineers of the eighteenth century and the splendid work which they did. But I must not turn aside to this tempting theme—and it must be said that these men worked for the most part empirically, and that, although machine science is enormously indebted to them, their influence on it was almost altogether indirect. It thus happens that the English engineering literature of the period in question scarcely calls for remark, in fact, it had scarcely any existence. The Registrar of the Society of Arts published, in 1772, a large volume of descriptions of machines and models, possessed and approved by this Society, and a second similar volume was issued in 1779; but these were purely descriptive, like the more extensive "Description des Arts et Metiers faites ou approuvées per Messieurs de l'Academie Royale des Sciences," which was published in Paris in 1761, in eleven folio volumes. Books treating engineering subjects scientifically, hardly ever appeared; probably the engineers themselves had neither the time nor the knowledge to write them, and certainly no one else had. The earliest record I can find of anything like systematic experiments on the strength of iron in this country, and one of the earliest anywhere, and the application of their results to machine design, is contained in a little book, published in 1803 by a Mr. John Banks, who styles himself "Lecturer on Philosophy." The author not only gives particulars of a number of experiments on a tolerably large scale, which he seems to have conducted personally, on beams of different forms, but indicates by numerous examples the application of his results to the designing of machine details. Banks adopts Galileo's erroneous theory as to the resistance of beams without question, but in most of the special cases of which he treats, the results are not much affected by it. He did not add anything to our knowledge of the theory, but his work, in rendering possible the application of theory, cannot be overlooked.

But Banks was certainly not the only one who was experimenting on the strength of iron at this time, any more than England was the only country in which such experiments were required. We find that the science of design—of proportioning form to stress—extended very rapidly everywhere, and was recognised as a promising field for further investigation. Olinthus Gregory, in his "Mechanics" (1806), devotes a good deal of space to it, though his adoption of Galileo's theory brings him into some queer mistakes; and Buchanan's "Essays on Practical Mechanics" (1808, and subsequent years), form early contributions to this part of the science. Both writers refer to Banks. Among the first, if not the very first, to state the theory of beams correctly, and at the same time in a manner accessible to others than mathematicians, was Prof. Eytelivein, of Berlin, in his "Statik festec Körper" (1808), who also pointed out that time was a most important element in all matters concerning the resistance of materials to load. In this country we owe a great deal to Tredgold in this connection. He was probably the first to give anything like systematic treatment of machine details in reference to their strength. This he did in his steam-engine, 1826. I have now traced the development of machine science up to a time within the memory of living men. It remains that I should point out its more recent course, and indicate so far as I can its present condition. From a purely scientific point of view this is the most interesting part of the subject, but to

treat it in a way corresponding to its scientific interest would require far more time even than I have already given to the more distinctly historical part. But although scientifically of so great interest, it could hardly be made interesting to an audience like the present, for anything like a full treatment of it would involve so many mathematical and technical details as to make it almost unintelligible to all but the initiated. I must, therefore, content myself here with a very brief review of the matter.

Looking first, then, at the mathematical side of the science, the first professors at the Ecole Polytechnique, to whom I have already alluded, made such enormous advance all at once that it was years before their contemporaries caught them up. Prof. Willis, in his "Principles of Mechanism" (1841), did yeoman's service, as every one knows, for his favorite subject, and his name is the most distinguished of those who have written upon it since the commencement of the century. He did not essentially alter its basis, and his system does not seem a satisfactory one, but his investigations in many directions were none the less extremely original and valuable. He has had innumerable followers both here and on the Continent, and among them Rankine is to be reckoned, whose original genius did good work here, as in other branches of machine science. Within the last few years, however, the orthodox faith in the old classification of mechanics has received a rude shock from the impetuous attack of Reuleaux, who maintains that they are, at the best, merely empirical, and who wishes not to modify, but entirely to subvert the former treatment of the subject. Reuleaux's leading idea is this:—The analysis of the machine may be carried further than the mechanism, which has hitherto been treated as an indivisible unit. The mechanism consists of certain elements arrayed in pairs—each pair constraining a particular motion (a pin and an eye, a screw and a nut, &c.)—these pairs are linked together by resistant bodies so as to form "kinematic chains," in which the relative motions of each link to every other are absolutely constrained, and the fixture of any one link of the chain constitutes it a mechanism. This method of analysis gives quite a new insight into the relations between one mechanism and another, and renders the treatment of the whole by one general method as easy as before it was difficult. Reuleaux has advanced the science of mechanism in other ways also, but this is his most important work, and one which, I believe, forms the most important step which has been made forward since the beginning of the century. It not only forms the basis of instruction in the principal German Polytechnic Schools, but forms also a foundation upon which already many men are doing valuable work.

The advancement of geometry told upon machine science. The growth of machine science has not been without its effect, direct and indirect, on geometry. Aranhold's "Kinematische Geometrie"—a little pamphlet, containing an amount of original matter altogether disproportionate to the fewness of its pages—we owe, no doubt, to the suggestions, of mechanisms; and still more recently the subject has been carried into higher branches in a most elegant manner by Burmester. These general investigations will react again upon our special cases, and will no doubt be found, as has so often been the case with investigations which at first seem purely theoretical, to have important applications even in the limited region of machine science.

I cannot leave this subject of the relations between geometry and machine science without some mention of graphical statistics. I intended to have spoken about it at some length, but time absolutely forbids me this. I must say only that while graphic methods of calculation had for many years been used to a limited extent, it is only very recently that they have been systematised; and so far as concerns calculations in which lines represent forces, this has been done chiefly by Culman Zurich, who has called his subject "Graphische Statik." He has treated it throughout in connection with the methods of modern geometry. The two studies are, I hope, inseparably connected, and the graphical statics, which should form an essential part of an engineer's education, may be thus the means of introducing into this country some of those modern geometrical methods which ought to form, so far as I am able to form an opinion, a part of the

education not of engineers only, but of every one.

In its physical aspects, machine science has, during this century, advanced alongside of physics. The most distinct progress which has been made here has been in the application of thermodynamics, and chiefly in the theory of the steam-engine. There probably was never a case in which the growth of a science and the growth of its children kept pace better. Rankine in this country, Clausius and Leuner in Germany, Hirn in France, as well as many others, whose interest in the subject was more purely physical, have worked so well that we have already a tolerably complete theory of heat engines, and are able to say in every case exactly what an engine ought to do, which by universal analogy must be the principal step towards making the engine do it.

The once popular study of hydraulics has become of secondary importance in connection with machinery, the steam-engine having to so enormous an extent superseded other prime movers. Redtenbacher's treatises on water-wheels and turbines, although published thirty years ago, remain the standard works on the subjects of which they treat. The more recent scientific research in this and allied subjects find its application chiefly in hydraulic engineering, with which we have not here to do.

And now, lastly, as to the progress of machine design, so far as it has been reduced to a science, and connected with the molecular resistance of materials. It was Redtenbacher who first fairly separated this study from mechanics, and systematised and extended it. Indeed it may be said that it was he who created machine design as a separate branch of science. His "Resultati für den Maschineu-bau" was published in 1848, and although much progress in details has been made since then, and his method of treatment may in many cases be greatly simplified, and must in some cases be entirely altered, this work and his published lectures ("Die Maschinen-bau"), remain classics. The direction of progress since his time has been in the more and more complete reduction, to a scientific basis, of the numerous determinations formerly made upon grounds of "experience" alone, that is empirically; and no year passes without some such gains to science at the expense of empiricism.

During the last thirty years a host of experimenters has been at work investigating the strength of iron and steel. As time has gone on the experiments have been made more and more complete and accurate, and in some particular branches of the subject the experiments may be said to be complete, so far as concerns the constructive materials now most used. We have, for instance, thanks in great measure to Mr. Kirkcaldy, little left to learn as to the ultimate tenacity of many qualities of iron and steel, and as to their behaviour when near the breaking point. But, unfortunately, many new fields for experiment are opening up. Engineers are not in the habit —intentionally, at least—of straining the members of their structures or machines to near the breaking point; and, as the behaviour of many materials, when exposed only to small stresses, differs greatly from their behaviour under larger ones, it is now recognized that our knowledge of the strength of a material is incomplete unless we know to what extent it is affected by such small loads as it usually receives in practice. New experiments, so far as they are to have scientific value, must now take this direction, and new apparatus is required for them, much more delicate in its operation than the former machines.

Modern experiments on the strength of materials have given us two conclusions, which have a specially important influence on machine design, and which I must not pass over without mention. The first of these is that the fracture of a material does not depend solely on the magnitude of the load resting on it, but also on the number of times which that load is applied. A bar of iron will be ultimately broken by many repetitions of a load of only about sixty per cent. of the load which will cause immediate fracture; and, as in most machines as well as structures the loads which do occur are repeated over and over again, it follows that the ultimate resistance of the parts of the machine to fracture is not much more than half what is indicated by the ordinary testing machine. The fact was established experimentally long ago by Fairbairn and others, and more lately very completely by the ex-

periments of Wöhler on the "Fatigue of Metals," which extended over some twelve years, and were conducted with specially constructed machines. In many cases, one single piece of material was subjected to many million (in one case once 130 million) repetitions of load. The second conclusion, also established by Wöhler, is of even greater importance. It is, that the resistance of material is not, *cæteris paribus*, dependent on the maximum load to which it is exposed, but to the difference between the maximum and minimum loads, that is, to the range of stress. This discovery has now been fully substantiated in principle, and there can be no doubt that the design of machinery as well as structures, so far as it depends on the strength of the material used, should be based on this law, and carried out by help of formulæ derived from it. Such formulæ have already been investigated, and some of them, (notably that of Launhardt and Weyrauch's addition to it) appear to represent very well the physical conditions of the case, so far as they have yet been determined. The Bavarian Government have already gone so far as to adopt what they should call "Board of Trade Regulations," upon formulæ of this kind. The experiments already made have not been, however, numerous enough to determine the constants required for these formulæ, and very much still remains to be done in this direction.

I am no prophet, and will only say one word as to the future of machine science. Judging from the direction taken by it hitherto, we may infer that as more and more work is done in it we shall be able to get more and more distinctly at the ideal of every machine we design, and to see more and more clearly the highest form of the object at which we are aiming. At the same time we shall understand more and more completely the physical and other conditions which prevent the actual attainment of our object in this form. The more accurately we know these conditions the more closely shall we be able to bring our machines to the best form attainable under them. More than this: as we get to know these conditions more and more accurately, we shall see to what extent they are essential, and how far we can remove them. In the one case we can admit them as modifying our ideal machine, in the other case we get over them with our practical machine. In both processes our practical work and our ideal are brought closer together. Already with some special machines, the real and the ideal have almost met. I look forward to the time—although we shall not see it—when the same condition shall have been reached in the complex machines of every-day life, and when the growth of machine science shall have made it possible to produce a steam-engine as perfect in its whole working as, say, the motions of Sir William Thomson's tide-calculating machine. "After this, the deluge."

FELSPARS, AND THEIR DERIVATIVES.*

By Professor SMYTH, F.R.S.

From the "London Mining Journal."

The felspars, though differing much among themselves, form a group of substances of which the world at large takes but little notice; and yet they are among the most important substances with which nature has endowed the earth. The term "spar" has been in use for a very long time, and was originally given to substances which in their purer condition have a glassy appearance, with a lustre somewhat inferior to true glass, with a certain amount of translucency, lightness, and color, and with a moderate hardness. Amongst these sparry substances the felspars were placed, the prefix denoting the particular kind of spar, being according to some derived from "feld" (feld-spar), inasmuch as the substances are usually found in mountains or hills; or,

* A lecture before the Royal School of Mines.

according to others, from "fel" (felspar), as being prominent constituents of rocks. Some varieties of felspar occur so frequently, and so well marked, that an observant person cannot fail to have noticed them. For example, in our London streets, in the crossing places, or in the kerbstones, where the rain has washed away the mud, and heightened the color of the minerals, large crystals will be seen, having a certain definite shape, and of white, brown, reddish, or yellowish colors. These may also be very distinctly seen in the magnificent stones which have been imported so largely into London of late years for the construction of several bridges (London Bridge, Waterloo Bridge, &c.) and of the Embankment. These conspicuous crystals have a peculiar lustre, and often exhibit this peculiarity in structure—that they appear as if a line were drawn down the middle of the length of the crystal, and the one half looks bright and the other dark, whereas from another standpoint the bright and dull sides will be reversed. The latter feature shows the crystal is what mineralogists often call a twin crystal. In the districts of Malvern, or Cornwall and Devon, in the highlands of Dartmoor, &c., we should find large blocks of rock to be composed to a very large extent of this class of substances.

Perhaps the most important of these is what is called "orthoclase," or common felspar. It derives its name from the fact that the crystals have two principal directions of cleavage, which form a right angle with each other. Its hardness is inferior to that of quartz, nevertheless it is greater than that of ordinary spars, since it will make scratches on glass; hard steel will touch it, but with difficulty; its specific gravity is moderate. The crystals in the granite used for the construction of the bridges, &c., referred to above, are mostly of orthoclase, and in these cases it will be seen that the crystals are embedded in a paste. To a rock of this kind the term porphyry is given; if, however, the crystals of felspar are not so very distinct from the rest, but two or three different substances are so distinctly crystallized out, the term porphyritic granite is often applied. Beautiful varieties of this porphyry may be seen in different places; thus the public buildings of Penzance, in Cornwall, are built of a beautiful material of this kind, and reference may also be made to the granite from Shap Fell, in Cumberland, which in the last five or six years has found its way into London for ornamental purposes—columns, &c.; some of the elements of its beauty and utility being its hardness, its pink color, and this porphyritic structure. In the Alps, again, and in the neighborhood of Lago Maggiore, large blocks of granite are thrown from the mountain side on to the plain below by small charges of powder, and often on examining these it will be found that in cracks and cavities the felspar, along with quartz and mica, is beautifully crystallized, sometimes on a large scale. Other notable localities are the Island of Elba, and the Mourne Mountains, in Ireland. In some instances the crystals of felspar are more translucent, looking more like glass; and being found in the Alps, in the vicinity of St. Gothard, they have been named "Adularia felspar," from a mountain there known as Adula. In the Island of Ceylon this variety of clear felspar has a tendency to appear rounded, and is found when cut to present a peculiar lustre, something like mother of pearl, hence it is usually termed "moonstone," and is much valued for ornamental purposes. Felspar is found in other cases with a yellowish, brownish, or reddish tint, and a green variety, from Siberia, has received the special name of "Amazon stone." Some beautiful examples of this latter variety have recently been found near the line of the Pacific Railway, and the Museum owes some fine specimens to the liberality of a lady. If we look at the composition of this orthoclase we find that it is a silicate of alumina, with a silicate of potash, one specimen giving on analysis about sixty-five per cent. of silica, eighteen per cent. of alumina, and fourteen per cent. of potash, with very small quantities of lime and soda. The presence of potash in this substance is of great interest to the agriculturist, since the soils produced by the decomposition of rocks containing orthoclase will be suited to certain plants, to the life of which potash is so important. There are cases where this kind of felspar constitutes an entire rock, as at St. Stephens, Cornwall. Certain building stones contain another variety of

orthoclase, known as "glassy felspar," and notable amongst these is the trachyte of the Rhine district, while the magnificent cathedral at Bologne may be pointed out to an example of its use.

To another specimen of felspar the name "albite" is given, in consequence of its whiteness. Crystals of this felspar occur generally on a smaller scale than orthoclase, and are almost universally twin crystals, often appearing as if the crystal had been cut in two and one half turned on the other, so as to form a re-entering angle. It is a silicate of alumina and soda, and thus differs from orthoclase in having the potash of the latter replaced by soda. The question arises whether the potash and soda from these two substances—both valuable materials—could not be economically obtained; and in certain cases this can be done. In certain porphyritic rocks these two species of felspar are found together. "Oligoclase" is another species, containing little potash, but much soda, and some lime; it derives its name from the fact that its cleavage does not produce right angles. The term "labradorite" is applied to still another species of felspar, which occurs abundantly in Labrador. Like the last two felspars, the crystals of labradorite belong to the anorthic system, or the system in which none of the three axles are at right angles. The crystals of labradorite present usually a grayish appearance, but when the light is seen reflected from them in a particular direction they show the most gorgeous colors, and at the same time a very delicate striation, which structure is intimately connected with the production of color. This labradorite is composed of a silicate of alumina, coupled with a silicate of lime, and a fair proportion of soda. Consequently, when the rocks of which labradorite is a chief constituent decompose, they give rise to soils rich in lime. If we look to the localities in which labrodorite occurs we shall find it is a very important material; it occurs abundantly in the basalt, augitic porphyries, and modern lavas of the Sandwich Islands, Iceland, Vesuvius, Etna, &c. And when these decompose, so far as to form soils, they often produce very rich soils indeed; and it is interesting to note that from soils thus formed in the vicinity of Vesuvius and Etna some of the most famous grapes and wines of the world are produced. "Anorthite" is a fifth species of felspar, and is so called from the fact that in the form of its crystals there is no right angle: it consists of a silicate of alumina and of lime, and occurs on a very small scale only. There is a question as to whether these are all distinct species of minerals, or whether some may not be merely mixtures of two or more species.

In the lavas of Mount Etna labradorite is very frequent, some of the more modern ones being composed almost entirely of labradorite and augite. And even in our own districts this mineral plays a very important part. The "whin sills" of the North of England, the great masses of dark igneous rock in the Dudley and Newcastle coal fields, and in the great tract which reaches from the Castle Hill at Edinburgh to the Castle of Dumbarton, these rocks, which are variously called plutonic, trappean, or volcanic, contain this felspar as a most important constituent.

And connected with the series of felspars there is the enormous economical importance of substances produced from some of them. In certain districts of Devon and Cornwall orthoclase occurs in a very soft and powdery condition. And about 100 years ago Mr. Cookworthy, an intelligent gentleman of Plymouth, and a Saxon chemist independently came to the conclusion that this kind of powder was the same material as the kaolin from which the Chinese manufacture their very best China. In England a pottery was established at Plymouth, and then Wedgwood and others took it up, and it has since become one of our staple branches of manufacture. From the discovery by the chemist alluded to sprung the famous Dresden manufacture. In the year 1800 the quantity of this so called Cornish clay exported from Cornwall was about 2,000 tons, in 1839 it had reached 7,600 tons, while last year the quantity was 108,000 tons. Another substance, known as China stone, was exported to the amount of 38,000 tons.

In Devonshire, on the east side of Dartmoor, is a deposit formed by nature of a clay very commonly called pipe-clay, of which 29,000 tons were produced; while if we pass still further eastward

we find that a variety of clay, known as potters' clay, was exported from Poole to the amount of 65,000 tons, while 60,000 tons of the same material were exported from Devonshire. In fact, as a whole, 2,000,000 tons of these different varieties of clay are produced. And there is no doubt that, as in the case of kaolin, they are all produced from the decomposition of felspar.

STRENGTH OF IRON AND STEEL CONSTRUCTIONS—WITH CALCULATION OF DIMENSIONS.*

Translated from the German of "Weyrauch."

II.

PERCENTAGE OF CARBON, &C.

What is meant by the terms wrought-iron, steel, and cast-iron is more easily felt than explained; a definition, correct to-day, may not be so to-morrow. The latest authorities say that wrought-iron should contain about $\frac{2}{3}$, steel from $\frac{2}{3}$ to 2, and cast-iron more than 2 per cent. of carbon. But steel is to be found with $\frac{1}{4}$ and less per cent. carbon, and wrought-iron with about 1 per cent. Again, it is said that steel, but not wrought-iron, can be hardened; but steel with much phosphorus and little carbon cannot be hardened; wrought-iron, and even cast-iron, under certain conditions, may be made harder.

Greiner, Director of the Bessemer Works at Seraing, and Phillipart gave this definition of steel as contrasted with wrought-iron: "By steel is meant that kind of iron which can be obtained by fluid processes, and which, on account of its consequent homogeneity and compactness, is capable of offering a greater resistance; and which is also, because of the method of production, more uniform, both in composition and behavior." This would exclude many products from the category of steel.

Benedict's definition of cast-iron, correct in the main, is this: "By cast-iron is meant that obtained directly from ores, which does not admit of being wrought or welded; which melts at a lower temperature, and which contains the greatest proportion of carbon and foreign matter." Either one of the constituents of this definition alone is insufficient; *e. g.*, wrought-iron and steel can be got directly from the ore by Siemens' process.

Chemically pure iron has hitherto been obtained only in small quantity; it can be made very soft or very brittle, and is hard to melt. Iron becomes technically useful by combination with charcoal. This amounts to from 0.1 to 6 per cent., in part chemically combined, in part as graphite. With regard to the two sorts of metal which receive the names of wrought-iron and steel, it may be said that in either, the addition of carbon has an effect upon strength similar to that due to passing the elastic limit, or to mechanical treatment; the hardness and ultimate strength increase; while ductility and power of resistance to shock and sudden stresses beyond elastic limit diminish. This is less observable in wrought iron, because of the influence of other substances and of the mechanical treatment. But with steel there is a limit, beyond which the ultimate strength, at least for tension and compression, diminishes; and with this the ductility, so that the properties of the metal approach those of cast-iron. The position of this limit depends upon the presence of other elements, and the influences considered.

Knutt Styffe thought that he had found the maximum ultimate tensile resistance of iron and puddled steel at 0.8 per cent.: of Bessemer and Uchatius steel at 1.2 per cent. The latter agrees with the experiments of Vickers, in Sheffield, according to which the maximum is at 1.25 per cent. Karsten says that steel hardens best, and has most tensile resistance at from 1.0 to 1.5 per centage of carbon. With a greater percentage the hardness increases, but the

* Strength and Calculations of Dimensions of Iron and Steel Constructions, with reference to the latest Experiments. By J. J. Weyrauch, Ph.D. Four folding plates. New York: D. Van Nostrand.

resistance becomes less; at 1.75 per cent. all welding quality is lost; at 1.8 per cent. it works under the hammer with great difficulty; at 1.9 per cent. it can be worked no longer; and at 2 per cent. it has reached the boundaries between steel and cast-iron; it cannot be drawn out at red-heat without cracking and breaking under the hammer. Bauschinger has made some very interesting tests of Ternitzer Bessemer Steel. The test-pieces were made for the purpose, and were of the same sort, but contained different proportions of spiegeleisen. The results for ultimate resistance were as follows:

K, %	Tension.	Formula (9).	Compression.	Shearing.	Bending.
0.14	4,430	4,435	4,780	3,410	7,920
0.19	4,785	4,510	5,390	3,710	8,600
0.46	5,330	5,270	6,330	3,585	8,340
0.51	5,600	5,480	7,000	4,020	9,300
0.54	5,560	5,620	6,110	3,930	8,550
0.55	5,650	5,665	6,170	4,000	8,825
0.57	5,605	5,765	6,550	3,645	9,600
0.66	6,295	6,245	6,550	4,280	8,600
0.78	6,470	6,995	7,305	4,140	8,750
0.80	7,230	7,134	9,670	4,820	7,645
0.87	7,335	7,640	8,940	5,000	7,650
0.96	8,305	8,340	9,890	5,820	8,480

The elastic limit increased from 2,950 to 4,870; 2,775 to 5,000; 3,750 to 4,425. Setting off the tensile resistance as ordinates to the percentages of carbon as abscissas, a number of points marked by a cross is determined, grouped about a curve (I), of which the equation is

$$t=4{,}350\ (1+K^3) \qquad (9.)$$

in which K means the percentage of carbon. By means of this equation the values in the third column of the preceding table are found. The results of other tests are shown, notation as follows:

+ The results obtained by Vickers;

○ By Styffe, with hammered Swed. Bess. Hogbo round steel;

□ By Styffe, with rolled Swed. Carlsdal Bess. square steel;

○u By Styffe, from rolled Swed. Uchatius cast-steel, round, Wykmannshyttan;

○k. By Styffe, with soft hammered Krupp axle-steel;

□t Bauschinger, with rectangular tie-bars of Ternitzer Bes. steel;

○t By Bauschinger, with round rods of Bessemer steel.

This shows that formula (9) corresponds fairly, not only with Bauschinger's results, but generally with mean ultimate resistances; and that important deviations may occur from various causes. The equation

$$t=3{,}700\ (1+K^3) \qquad (10.)$$

which corresponds to curve II gives results, below which in general the ultimate resistance will not fall.

With respect to the results obtained for compression in the above table the following must be noted. Test rods from 3 by 3 by 9 cm. were strained between two compression plates. With the increase of load an S-formed curvature was observed, which increased more and more, till the prism suddenly sprung out. The strain on the fiber at the moment of springing is regarded as the ultimate resistance.

Bauschinger's tested pieces were of the form shown above.

The load was increased, and a pressure was reached without further increase, under which the prism contracted in length to less than half, while the transverse dimensions increased. The stress per square cm. at this limit, which Bauschinger regarded as the ultimate strength, increases with the percentage of carbon, from 9,250 to 17,800. On the other hand, the elastic limit was independent of the kind of test. Generally very short steel prisms may be loaded to double the amount permitted for tension.

Phosphorus, like carbon, increases the elastic limit, and ultimate tensile resist-

ance, but diminishes the power of resistance to blows and to stress differences. It makes iron brittle, coarsely crystalline, and "cold-short," that is easily broken under cold working. For this and other reasons it cannot be used in bridge structures. Phosphorus affects steel still more unfavorably than iron. According to Greiner, steel, with from 0.2 to 0.25 per cent. of phosphorus, has too little strength for technical purposes. Phosphor-steel is best suited for rail-heads, because it resists wear; but the percentage of carbon should be diminished to prevent brittleness.

According to Sandberg and Turner, silica has the same effects as carbon, while Haswell, in the case of steel, with a certain proportion of phosphorus, ascribes it to a partial neutralization of the bad properties due to the latter. Slag helps phosphorus iron by diminishing its brittleness; but it makes it hard to work without splitting and springing. Next to phosphorus, sulphur is the most undesirable ingredient, having a like effect, except that it makes the metal particularly apt to break at red heat. Manganese, too, is a bad ingredient.

The effects of the above mixtures and others upon the strength of iron and steel are not clearly determined. Concerning their effect in the foundry information can be had from any text-book upon Metallurgy.

Whether, in a given case, steel or iron is to be preferred, depends upon considerations of resistance to special strains, of lightness, security under changes of temperature, economy, &c. In the application of steel, the proper percentage of carbon is dependent not only on the mechanical working it is to undergo, but also upon the composition of the ores and the method of production, because the proportion of other ingredients is determined by these. So Vickers recommends for pieces subjected to both tension and shock, 0.62 to 0.75 per cent.; Styffe, for axles of Swedish steel welded, or of one piece, 0.4 to 0.6; Greiner, for axles of Bessemer steel from Seraing, 0.3; Krupp, for locomotive and marine-engine axles, 0.5 to 0.6; for coach axles, 0.6; Greiner assigns for Seraing Bessemer steel, for chains and driving rods, 0.25 to 0.35; for tires not welded and piston-rods, 0.35 to 0.45; for steel rails, 0.4; for springs, 0.45.

INFLUENCE OF TEMPERATURE.

The influence of different temperatures upon the strength of steel and iron is not satisfactorily explained. With respect to ultimate resistance only, because of numerous experiments, has there been a growing accord of views. For most kinds of metal, especially for iron, the ultimate strength appears to increase with the decrease of temperature below zero, but also to reach a maximum at a little above 100 C. Within a certain interval near 16° the resistance is quite constant; the beginning and the rapidity of the increase and the position of the maximum are dependent upon the conditions already considered.

Fairbairn, in tension experiments with bar iron, found, in one case, the resistance at 0° equal to, in another, 1 per cent. higher than at 60°. Thurston found in torsion experiments a decided increase of strength to — 12°. Spence, in experiments in bending cast-iron, found at — 18°, a strength greater by about 3.5 per cent. than at + 15°. At higher temperatures, Fairbairn found for bolt iron the maximum of ultimate tensile strength at 163° 41 per cent. greater than at 18°; later experiments with bar iron put the maximum at 213°. A commission of the Franklin Institute, at Philadelphia, found the maximum strength 15 per cent. greater than its ordinary value at about 288°. Styffe has published the results of numerous experiments. See his Table VII.

Beyond the maximum the ultimate resistance decreases at first slowly, but very rapidly at red-heat. In this respect, too, the different kinds of metal behave very differently, and the diminution may possibly be the quicker and more rapid the lower the temperature of the metal when under mechanical treatment. Tensile resistance Fairbairn found to diminish from 202°, where it was about the same as at ordinary temperature, to a low red heat, by about 17 per cent.; up to ordinary red heat, by about 34 per cent. Experiments at the Franklin Institute found the ultimate tensile resistance, at 575° lowered by 0.66, and at 700° by 0.33 from the ordinary value. Bauschinger observed the strength of

puddled plate, transverse to the direction of rolling, to be at red heat 780 kil. (2,700 ordinary), and of rolled iron along the fibers, 750 (4,430 ordinary).

These results are of importance with respect to constructions exposed to fire. Kirchweiger, of Hanover, regards the diminution of tensile strength by heating as the cause of boiler explosions; attempting to prove at the same time that a boiler filled with water may become red-hot. Bauschinger thinks it possible that the continual variation and differences of temperature of the outer and inner surfaces may diminish the cohesion of the laminæ of the plate; the inner laminæ bearing a disproportionate share of the strain, and the shearing resistance being lessened.

A frequent theme of discussion is the influence of cold upon resistance to sudden changes of stress,—shocks in particular. It cannot be denied that more axles and wheels break in winter than in summer. Styffe maintains that rupture is often due to the fact that the parts are held fast, and, therefore, cannot yield to the contracting influence of the cold: again, for tires, axles and rails, the effect of shocks is increased by the diminished elasticity of the ground.

Sandberg, in an appendix to the English translation of Styffe's work, maintains that these are not the principal causes of breaking. He laid iron rails upon granite supports which lay upon granite rocks, so that the elasticity of the foundations might be the same in any season. The two halves of these rails were tested by blows with a 380 kil. ball at $-12°$ in winter, and $+29°$ in summer; and it was found that at $-12°$ the rail could withstand only $\frac{11}{39}$ of what it could at $+29°$. This showed, at least, that there are some kinds of iron that are weakened by frost. Styffe had tested only under dead loads, and in this respect his results were trustworthy.

Sandberg also found this peculiar result: that Aberdare rails, which bore in summer 20 per cent. more strain than those from Creusot, in winter had 30 per cent. less strength. This could be explained on the hypothesis of a difference in constitution which affected the strength unequally. Fairbairn had already shown the unfavorable effect of phosphorus and sulphur at low temperature; and Sandberg thought it possible that different results would have been reached had the metal been free from phosphorus.

Unfortunately the chemical constitution of the rails was not determined; but it seems likely, that phosphorus, which always diminishes resistance to shock, may operate more actively at a low temperature. Its effects also increases under high heat. Styffe found that the grain of a screw-bolt of phosphor iron was so affected, that a single blow of the hammer broke it. Steel, with increasing mixture of phosphorus, loses its capacity to undergo repeated heating without losing its peculiar properties.

In the year 1871, Joule, Fairbairn, Spence and Brockbank contributed to the Manchester Literary and Scientific Society four papers upon the influence of cold upon iron and steel. All agreed that resistance to dead load was not diminished by cold, but considerably increased. Brockbank held it certain that cold diminishes resistance to shock; this, Joule and Fairbairn did not admit. All referred to experiments. No one will question the exactness of Joule's tests; but the test-pieces were wires, needles and nails, so that the results may not hold for larger pieces; while Fairbairn and Spence tested only under dead load. A series of observations by Brockbank confirm the results obtained by Sandberg. Rails were tested with blows; and in frosty weather they had far less strength than at ordinary temperature: a hollow cast-iron core-rod, about which a cylinder had been cast, cooled down to $-7\frac{1}{2}°$, broke square and smooth, leaving a brittle-looking surface, while the pieces were made stiff and sound again by heating. A rod of round-iron of best quality, of 38 mm. diameter, which lay a week exposed to frost and was covered with ice, broke at $4\frac{1}{2}°$ under a single blow of a hammer weighing 5.4 kil.

All authorities admit the increase of resistance to tension under great cold, though they deny that there is a diminution of power to resist shocks. This is bad reasoning. It is certain that resistance to dead load is somewhat increased by frost; and besides this, according to Styffe, the elastic limit; just as in the

case under hammering, rolling, hardening, &c.; but as with all the latter, resistance to shock increases, there seems to be no reason for a contrary judgment in the first case. Styffe has proved that iron becomes stiffer with decrease of temperature; agreeing with Sandberg.

Thurston concludes from results of his experiments that phosphorus and other substances, inducing cold brittleness, may impair resistance to shock at low temperatures, which seldom occur; and that in other cases resistance to dead load, as well as to shock, is increased by cold. This would be novel, but it must first be proven. Thurston's test-machine is well adapted to the lecture-room, being convenient and cheap; but it is not suitable for scientific experiments requiring results numerically exact. The velocity, an important element, is not regulated; the methods of measurement are much too primitive to answer to small differences due to temperature; and it is not to be taken for granted that torsion-tests are best suited to determine the properties of resistance of fibrous and laminated metals.

In a report of the Massachussetts Railroad Commissioners (1874), mentioned by Thurston, it is said, that "cold does not make iron and steel brittle and unsuitable for mechanical purposes, and that it is not the invariable rule that the most breakings occur on the coldest days." The membership of the Commission is not given, nor is it certain what kinds of metal were under consideration. Did it contain a large percentage of phosphorus? Were the rails iron or steel? It has been found in Northern climates—Canada, Sweden, and Russia—that a low steel, with $\frac{1}{3}$ to $\frac{1}{2}$ per cent. phosphorus, was affected by cold much less than iron. According to Styffe, there is no authentic case in which good steel contained more than 0.04 per cent. of phosphorus; though in one English iron rail there was 0.25 per cent., and in Dudley iron 0.35.

We draw the following conclusions from all the data at hand: (*a*) Iron and steel, which are entirely or nearly free from all foreign materials, have neither their resistance to dead load notably increased by cold, nor their resistance to shock diminished. (*b.*) Certain elements, not exactly determined, but phosphorus certainly, very much diminish resistance to shock and sudden change of stress. (*c.*) The question cannot be definitely settled until the chemical constitution is determined. (*d.*) Statistics of results in warm and cold latitudes, in summer and winter, after long frost, on days of sudden intensity of cold, are required.

The above has reference to the immediate influence of temperature. In regard to the effect of repeated changes of temperature, Wohler conjectures that frequent vibrations of molecules caused by heat, have the same effect in destroying cohesion as vibrations caused by external forces. Data from observation have not been obtained. Spangenberg, after examination of the fracture surfaces, did not adopt this hypothesis. Bauschinger, after testing boiler-iron, thought it possible that the strength of the plate was weakened by long action of the fire. But this decides nothing as to the effect of repeated influences. If Wohler's hypothesis is correct, we should recognize in change of temperature a cause of destruction, not only of metals, but also of all other solid bodies. And safety coefficients would be of no avail, but if we should make one beam twice as large as another, each half of the first would be as much affected as the whole of the second. In any case, bridges and buildings, which are subjected to only slight variations in temperature, will certainly be more likely to fail from other causes.

Bauschinger found the ultimate bending strength of steel, *i.e.*, the greatest fibre-tension at the instant of rupture, as given by the ordinary theory, always greater than the absolute tensile resistance. Wohler obtained a like result for wrought iron and steel; but the original strength was not less for bending than for pull. The experiments of Bauschinger and Styffe show that the modulus of elasticity for bending may be assumed as equal to that for tension, without great error. All these results show that the common theory of bending gives results accurate enough for practice. Of especial interest in this respect are Bauschinger's tests, in which the length of the gravity axis or elastic line remained unaltered by bending, and the original plane transverse sections remained perpendicular to it, even under very strong bending stresses.

Though it is not asserted that the method of calculation for very thin-walled plate-girders is exact in every respect; yet it is as sound as that for trusses, in which hinges are supposed, but rivets used; and it is safer than the ordinary method for compound trusses.

The modulus of elasticity of steel per square centimeter, is, according to

Bending tests by Kupffer, 2,124,990 (cast and file steel).

Pull and bending tests by Styffe, 2,412,300 (Bessemer steel).

Tensions tests by Bauschinger, 2,215,500
Compression tests by " 2,391,000
Bending tests by " 2,110,000
(Best steel prepared for test).

Crushing tests by Bauschinger, 2,082,500 (Best round rod).

Tension tests by Bauschinger, 2,310,000 (Best tires).

Bauschinger found the elastic modulus for torsion and shearing to be 862,000. From these results it follows that for steel we may assume as average:

For tension, compression and crushing E, = 2,150,000.

For shearing and torsion $E' = \frac{2}{5} E =$ 860,000.

In experiments with English tire iron, bar iron and Swedish wrought iron, Kupffer gets a mean of 2,053,070; Styffe gives for good iron, with very little phosphorus 2,171,100; but for iron containing much phosphorus and slag, 1,930,600. The following figures are established for iron:

For tension, compression and crushing, E=2,000,000.

For shearing and torsion, $E' = \frac{2}{5} E =$ 800,000.

No effect of carbon upon the elastic modulus could be observed; but with Styffe and Kupffer, it seemed to increase a little with the specific gravity and with lowering of temperature. Passing the elastic limit, and working in the cold condition, were found by Tresca and Styffe to cause a decrease.

According to Kupffer, hardening of hard steel decreases the elastic modulus by about 6.5 per cent.; but on the other hand Morin ascribes to cast steel a possible increase of E by hardening, by fifty per cent. By Wertheim's theory and Kirkaldy's tests, the specific gravity is somewhat diminished, if the metal is worked cold or in any way the elastic limit is passed, while the volume does not decrease, as has often been assumed. Yet all these influences are not so great and well determined that they require or permit a general review.

In calculations, the specific gravity of wrought iron may be put at 7.6 to 7.7, that of steel 7.8.

THE EXAMINATIONS OF METALS.

The higher the limit of elasticity, the greater the strain which a body will bear without permanent change of form. Raise this by hammering or hardening, and the body will be restored after greater strains; hence the extended use of springs. If the ordinary elastic limit served for all kinds of load, and if we were sure that it would never be exceeded, then it would be desirable to set the limit as high as possible for any construction. But the ordinary value is not sufficient in case of shock. In our riveted bridges, for example, local excesses may occur, because of unequally distributed strains. These are less dangerous, if the material is strong enough beyond this limit, so that a gradual change of form takes place, as in the case of a uniformly distributed force over the whole section.

The more extensible and tenacious the metal, the less risk in exceeding the elastic limit. It is well known that a very ductile and tough metal best resists shocks and sudden changes in stress. We should, therefore, judge of the fitness of metal, not only by the height of the elastic limit and the ultimate resistance, but also by its ductility and tenacity. The greater the latter qualities the greater the elongation before rupture.

When a rod is broken by a pull, there is a contraction of section at the breaking-point, beginning a little before rupture; attended by a decided elongation, which is independent of that which always occurs when the elastic limit is exceeded, and is approximately proportional to the length of the rod. As the total elongation at rupture is in part proportional to the length of the rod, in part independent of it, the ratio $\frac{\lambda}{l}$ of the total

elongation to the length of the rod, can determine the ductility only in the case of rods of equal length; for the shorter the rod, the greater relatively the share of elongation at the point of rupture.

Kirkaldy, who has had the advantage of very many tests in this regard, recommends that we measure the excellence of the metal, both by its ultimate tensile resistance and by its contraction at the point of rupture. The stress at the breaking point, per square unit of the contracted part, increases with both the tension and the contraction; and the stress at this time furnishes the best means of determining the resistance. The results so obtained, arranged in order, give a trustworthy scale of values; but, if the gradation were according to ultimate strength only, very ordinary kinds might stand high in the scale. Kirkaldy found that the ultimate strength of coarse, crystalline metal, was equal to that of very tough and dense sorts.

The mechanical treatment and the method of production have their influence. So plate-iron is generally of less ultimate strength and ductility than round-iron.

The Department of Public Works, in India, has published the following table of requirements for estimate and supply, based on Kirkaldy's results. Contraction is expressed in per cent. of the original cross section:

	Class C.		Class D.		Class E.		Class F.		Class G.	
	t for Tension.	Cont.	Ten.	Cont.	Ten.	Cont.	Ten.	Cont.	Ten.	Cont.
Round and Square Iron..	4,250	45	4,092	35	3,937	30	3,775	25	3,620	20
Flat Bar-Iron	4,092	40	3,937	30	3,775	25	3,620	20	3,466	16
Angle and T Iron.....	3,937	30	3,775	22	3,620	18	3,466	15	3,300	12
Plate Longit.....	3,755	20	3,620	15	3,466	12	3,300	10	3,150	8
Plate Trans.....	3,466	12	3,150	9	3,000	7	2,830	5	2,675	3
Plate; Mean	3,602	16	3,375	12	3,233	9.5	3,065	7.5	2,912	5.5

These figures show that we should use as much flat bar-iron as possible in our bridges for the parts under tension. Round-iron is useful in roof trusses.

In America, the conditions of proposals for bridges require high figures for ultimate strength (generally from 3,900 to 4,200 kil.); and test-bars must also stretch from ten to fifteen per cent. of their length before rupture. An elastic limit of from 1,600 to 1,750 kil., and uniformity of elastic modulus are prescribed. For example, in the case of the new Ohio bridge, no deviation of more than ten per cent. from the mean modulus of elasticity is allowed. Besides this, each piece under tensile strain is subjected to a test of twice the strain calculated for it—*i.e.*, about 1,400 kil. per square centr.—and, while under this strain, it receives a heavy blow from a hammer. It is generally thought in Europe that the Americans subject their bridges to a much greater strain than we; but for $b=700$, it amounts to about the same.

It is obvious that for the same ultimate strength the original strength increases with the extensibility, whether u is greater or less than the ordinary elastic limit. That the latter is possible follows from the fact that Tresca could push the elastic limit nearly up to t, and that permanent changes of form occurred below the elastic limit; and that in general the ordinary elastic limit has no influence upon many kinds of stresses. It is not impossible that at some time there will be found a sufficiently determinate relation between original strength and ultimate strength and contraction; or, between the ultimate strength and strain per square unit of the rupture-surface; or generally between u and values under dead load; so that u can be at least approximately found for each metal, and the numerical values be substituted in Launhardt's formula. And the vibration-strength s could be derived from some relation, or might be estimated Wohler found the rates $\frac{s}{u}$ nearly the same in metals so unlike as Phœnix iron and Krupp's

cast-steel; the values being respectively $\frac{7}{12}$ and $\frac{8}{12}$. It would be desirable to make a great number of tests by bending, shock, &c., of metals for which the values of t, u, and s, have been fixed by numerous experiments. We should then have a better guide for the tests required of the manufacturers.

PERMISSIBLE STRAIN.

The values of the stresses having been calculated, the working strength a gives the stress per square unit, which can be maintained without rupture, under any number of repetitions. No reference is made to influences that do not admit of systematic investigation, such as shocks due to the passing of wagons in the streets, flaws, rust, &c.

A WROUGHT IRON.

Tension or Compression only.

For Phœnix axle-iron, Wohler's tests give $t=4{,}020$, $u=2{,}195$; and the working strength for bending

by formula I $a=2{,}195\left(1+\frac{5}{6}\frac{\text{min. }B.}{\text{max. }B.}\right)$

Calculation must be made for the most unfavorable strain. For the same iron, under the ordinary strain, $u=2{,}195$, and $t=3{,}290$. This shows that such axle-iron is a metal which can hardly be suited for bridge-building. If no greater value is given, we put

$$\frac{t-u}{u}=\frac{3{,}290-2{,}195}{2{,}195}=\tfrac{1}{2},$$

and $$a=2{,}100\left(1+\tfrac{1}{2}\frac{\text{min. }B.}{\text{max. }B.}\right)$$

Taking $\frac{1}{3}$ as safety co-efficient, the permissible strain per square meter,

$$b=700\left(1+\tfrac{1}{2}\frac{\text{min. }B.}{\text{max. }B.}\right) \qquad (11)$$

Alternating Strain.

For Phœnix iron, Wohler found $u=2{,}190$, $s=1{,}170$; hence

$$\frac{u-s}{S}=\tfrac{7}{15} \text{ and}$$

by formula II, if $\frac{1}{3}$ be the co-efficient of safety, we find in round numbers

$$b=700\left(1-\tfrac{1}{2}\frac{\text{max. }B'.}{\text{max. }B.}\right) \qquad (12)$$

Here max. $B >$ max. B'; both values numerical, without sign.

Special Cases.

For pieces continually under dead-load we find from (11), since min. $B=$ max. B,

$b=1{,}050$ kil.

For pieces always strained in one direction, then restored to strainless condition, since $B=0$,

$b=700$ kil.

For bridge and roof girders, if p is the weight of structure, and q the total load, per running meter,

$$b=700\left(1+\tfrac{1}{2}\frac{p'}{q}\right)$$

For parts in which maximum tension and compression are equal, $b=350$ kil., by (12).

B. STEEL.

Tension or Compression only.

For Krupp's cast steel, Wohler found $t=7{,}340$, $u=3{,}510$. Reducing the value of u somewhat, because the differences in strength of steel are considerable, and introducing the safety factor $\frac{1}{3}$;

$$\text{since } \frac{t-u}{u}=\tfrac{7}{6}$$

$$b=1{,}100\left(1+\tfrac{6}{11}\frac{\text{min. }B}{\text{max. }B}\right) \qquad (14)$$

This gives three-fold security if $t=6{,}000$ and $u=3{,}390$. This value of t by formula (9) answers to a steel of about 0.6 per cent. carbon, which is suited to bridges. Wohler found u for axle steel of Krupp, Bochum, Seebohm; and Krupp's plate steel between 3,300 and 3,500; for spring-steel, not hardened, of Mayr in Leoben and Krupp, 3,650. Of course the best material should be used for bridges, and it should not contain more than 0.03 per cent. of phosphorus.

Alternating Strains.

For the same cast axle-steel, Wohler found $s=2{,}050$; if $u=3{,}510$, say 3,300, and safety factor is $\frac{1}{3}$;

$$\text{since } \frac{u-s}{u}=\tfrac{5}{12}$$

$$b=1{,}100\left(1-\tfrac{5}{11}\frac{\text{max. }B'}{\text{max. }B}\right) \qquad (15)$$

This gives three-fold safety for $u=3{,}300$, below which value the original strength of steel did not fall in Wohler's experiments, and for $s=1{,}800$, while in Krupp, Borsig and Bochum axle-steel the vibration-strength was about 2,000.

Special Cases.

For permanent strain under constant load (14) $b=2{,}000$ kil. For parts always strained in the same direction, then restored, $b=1{,}100$ (14) and (15). For bridge and roof girders, and generally for pieces for which

$$\text{min. } B \div \text{max. } B = \frac{p}{q}$$

$$b = 1{,}100\left(1+\tfrac{9}{11}\frac{p}{q}\right)$$

For parts under equal max. tension and compression $b=600$ kil. (15).

C. REMARKS.

The safety factors, and permissible strains for steel and iron, have special reference to bridges and large structures. Hitherto the permissible strain for wrought-iron has been set at 700. But it is found that b, for wrought-iron, may vary between 350 and 1,050. The most favorable case is that of dead load, the most unfavorable that of alternating tension and compression. In this we see how variable are the figures required for the safety of the different parts of a structure. Hitherto much material has been wasted in building. It is of no avail to the general security of a structure to employ 700 in places where from 700 to 1,050 may be required, and then to employ 700 in a place where 350 is ample. If there is only one diagonal or vertical in a bridge, which suffers nearly equal strains of compression and tension, the security is only half as great as has been assumed up to the time of Wohler's investigations. It would certainly be wise to strengthen such exceptional weak points, and so strengthen the entire structure.

The above values of b, for wrought-iron, give three-fold security if $t=3{,}150$, $u=2{,}100$, $s=1{,}050$, Wohler puts 1,100 for permanent structures, in case of alternation of strained and strainless conditions, under tension only, or compression only; and 580 for equal tension and compression; the previous figures being 700 and 350. These correspond to a safety factor of $\frac{1}{2}$. For temporary structures, the values of u and s are greater than we have assumed in (3). For the present this will answer, by taking all values as given above, and selecting another safety factor, say one-half under favorable conditions.

We have not derived the value of b from Wohler's tests of Krupp's spring steel, because the values of u, s, and t are not all determined, and because the steel had properties which can be assumed only in exceptional cases. Softer and more extensible metal will always be used for bridges. If with this, the ultimate resistance diminishes, it does not follow that it does so in the same ratio as the original strength; for this depends also on the ductility. In the case of hardened spring steel, with diminishing t, $\frac{u}{t}=\frac{1}{2.50}$: for steel not hardened, $\frac{1}{2.20}$; for cast axle-steel, $\frac{1}{2.08}$; and for iron, $\frac{1}{1.83}$ to $\frac{1}{1.5}$.

Hence estimate of working strength depends upon this. For Krupp's spring-steel, Wohler's bending tests give

$$a = 3{,}650\left(1+\tfrac{5}{6}\frac{\text{min. } B}{\text{max. } B}\right)$$

for the same hardened,

$$a = 4{,}390\left(1+\tfrac{3}{5}\frac{\text{min. } B}{\text{max. } B}\right)$$

If very low steel is used for a bridge, the permissible strain must be less. For example, if only 0.45 per cent. carbon is desired, and a minimum ultimate strength of about 5,200 kil. is prescribed, (14) and (15) may be changed to

$$b = 1{,}000\left(1+\tfrac{3}{4}\frac{\text{min. } B}{\text{max. } B}\right) \qquad (14a)$$

$$b = 1{,}000\left(1-\tfrac{1}{2}\frac{\text{max. } B'}{\text{max. } B}\right) \qquad (15a)$$

These formulas for $t=5{,}200$, $u=3{,}000$, $s=1{,}500$ give three-fold security.

For the arch-bridge at the Champ de Mars, of Bessemer steel, the permissible stress for all parts, whether under tension or compression, was put at 1,000 kil. There are smaller cast-steel bridges in Holland and one of puddled-steel in Sweden. The most important is the bridge at St. Louis, over the Mississippi, which has a middle span of 158.5 m., and two end spans of 152.4 m. The advantages of steel are its greater security against intense cold and its lightness. The difference in expense will not long stand in the way, as the cost of steel is diminishing.

RECENT EXPERIMENTS IN THE FLOW OF WATER IN RIVERS AND CANALS.

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Translated from "Der Civilingenieur."

Since the publication of the great works of Humphreys and Abbot in America, and of Darcy and Bazin in France, the first containing the extensive hydrometric observations on the Mississippi, a giant stream with an extremely small slope, and the second giving very carefully made experiments in small artificial canals with large slopes, hydraulic literature has been mainly occupied in discussing and testing the laws developed by those authors and deducing from experiments empirical formulas for the flow of water in rivers and canals. The theory of Darcy and Bazin which introduces the irregularity of the river bed as an important factor has especially been received with favor, and has in Switzerland been further extended by Ganguillet and Kutter. Nevertheless, it must be confessed, that the formulae proposed rest on a very insecure theoretical foundation and can scarcely claim to be more valuable than mere interpolation formula. Since then even our greatest hydraulicians are more or less in a fog with reference to the flow of water in rivers and canals, and as the practical engineer is yet waiting for further experiments to enlighten his way, it may be proper and profitable for us to glance in review at the latest publications on this subject.

GERBENAU'S EXPERIMENTS ON THE RHINE.

The first book we notice* treats of the international hydraulic survey of the Rhine, originated by a congress of engineers from Switzerland, Baden, France and Bavaria, held at Basel on November 1, 1867. The observations resolved to be undertaken included not merely the measurement of the velocity and discharge of water, but the comparison of different methods and instruments under similar conditions. The experiments were to be made at high, mean and low water; systematic gauge readings were to be taken at different stations as far down the river as Mannheim, and also simultaneous observations of temperature and rainfall.

Unfortunately this programme was only partially carried out, so that the results obtained occupy but a small space when compared with the extensive labors of Humphreys and Abbot. The study of Gerbenau's book is, however, very profitable, for it gives a most thorough account of the experiments undertaken, and contains many important limits on hydraulic measurements.

At Basel where the Rhine makes a sharp bend from the east toward the north, having for a considerable distance a rocky bed, it was not easy to find a suitable place for the proposed experiments. The locality chosen—about 120 meters above the upper suspension bridge—was in a slight bend where the thread of the current coincided nearly with the middle of the stream, the bed being formed of cobble stones varying from five to sixteen pounds in weight. Besides the main section for experiments, there were two others, one above and the other below, all at right angles to the thread of the current, their distances apart being 70.65 meters on the right bank and 90 meters on the left. The area of the upper section was 438.011 square meters, of the middle or main section 426.152 and of the lower section 420.795, all taken at a reading of five feet on the Basel gauge. These sections were measured by help of a line extended across the river and supported by three boats, along which a fourth boat took soundings at distances of from three to six meters. The profile of the banks was also accurately determined. A gauge was established at each of the three sections in order to note the oscillations of the river. In spite of the advanced season (Nov. 5 to 14, 1867) the weather was favorable, the water low and but slightly subject to rise and fall.

The principal velocity measurements were made by a Woltmann's mill manu-

* H. Gerbenau. "Die internationale Rheinstrom-Messung bei Basel vorgenommen am 6-12 November, 1867." Munchen, 1873.

factured by Ertel & Son of Munich. The wheel which was 0.19 meters in diameter, had two arms, each 4.5 centimeters long, and 7.5 centimeters wide at the ends, making an angle of 45° with the axis. The mill was fastened to the end of an iron bar in such a way, that its axis when immersed in the water would assume the direction of the current. The observer stood upon a stage supported by two boats from the up-stream side of which he could place the mill at any depth in the water, while the boats were secured by anchors and their exact position noted by measurements made at fixed points on shore.

For the details of construction of the apparatus and the manner of conducting the experiments we must refer to Gerbenau's book. It is to be wished, however, that more exact information regarding the adjustment of the mill and the determination of its constant had been given. The author seems to be of the opinion that the ratio of the number of revolutions of the wheel to the velocity of water is constant; although his experiments in Germersheim harbor had shown that this ratio decreased from 0.6148 to 0.5263 as the velocity increased from 0.4 to 2 meters per second, he regards it as constant for greater velocities than two meters. Since the wheel made 190 revolutions in fifty seconds at a velocity of two meters, while in the Rhine measurements 250 revolutions in 50 seconds were recorded, further experiments certainly seem to have been required for the determination of this ratio. Moréover the formulæ deduced appear somewhat complex, and do not seem to have required the calculation of coefficients to four decimal places, since the velocity measurements cannot be accurately taken to a tenth of a millimeter. Each observation with the mill lasted two minutes and was repeated three or four times.

For the measurement of the surface velocity Gerbenau used floats also. These were in part wooden cubes of 0.3 meters on an edge, which would float entirely immersed, and in part short sticks immersed from 0.6 to 0.9 meters. Observations were taken in groups of 10 to 20, the floats being dropped from a boat stationed above the measuring sections, and their course for a distance of ninety meters determined by plane tables on shore. At a distance of forty-five meters on each side of the main section where the mill measurements were taken, secondary sections were established. Two observers in the line of these sections signaled the passage of each float, while a third noted the time on a watch beating seconds, and a fourth marked the point of passage on a plane table. The mean of the times for each group was taken and divided by the distance between the sections to obtain the mean velocity, which was considered as belonging to a point in the river corresponding with the mean of all the positions where the floats crossed the middle section.

It might be objected to this process that the mean velocity should be properly determined by finding the velocity of each float and taking the average of all the velocities; since, however, the distance traveled by each float could only be closely found by recording the point of passage over the middle section, the simpler method of the author gives, perhaps, results sufficiently accurate. It might, indeed, have been satisfactory enough if the point of passage over the middle section alone had been noted; for the manner used by the author in finding the path of the float can give accurate results only by exception.

With the Woltmann's mill surface velocity measurements were made at five points in the main section with the Basel gauge reading 5.625 feet, and at four points with a reading of 5.175 feet. With the floats nine groups of measurements were taken at a gauge reading of 4.85 feet. From these three surface velocity curves were drawn, from which by interpolation a curve of surface velocity for a gauge reading of five feet was deduced. The five points of the first series of observations are so distributed that they afford a pretty accurate construction of the curve, although a sixth point near the right bank is much to be desired. The second series contains only four points in the center of the stream, and even the third series is deficient in points near the banks. It is hence not possible to draw the curves with the requisite degree of accuracy, and particularly it appears doubtful whether the velocity at the banks can be taken at zero, as is here assumed to be the case.

It may also appear doubtful whether the float observations should be directly compared with those of the mill, since the first were made at a depth of 0.15 meters below the surface and give the mean of velocity of the water for a distance of forty-five meters on both sides of the main section, while the last were taken at a depth of 0.25 meters and in the main section itself.

Further observations with the mill were made at six points to determine velocities at different points below the surface. These were taken on different days and at different stages of the water. For each vertical the results were plotted and through the points the so-called parabola of vertical velocities drawn. The area of each curve divided by the depth of the water gave the mean velocity at each vertical; the position of this and its ratio to the surface velocity received extended discussion. The number and position of the observations at each of the six verticals is given by the following table:

No. of Vertical.	Distance from Left Bank.	Gauge Reading.	Depth of Water.	Depths at which Observations were taken.
1	7.30 meters	5.70 feet.	1.93 meters	0.25, 0.60, 1.12, and 1.67 meters.
2	54.27	5.12	2.85	0.25, 1.65 and 2.65.
3	79.98	4.70	2.65	0.25, 0.75, 1.54, 2.0 and 2.45.
4	82.90	4.55	2.60	0.25, 0.60, 1.50 and 2.30.
5	145.25	4.42	1.90	0.25, 1.10, 1.40 and 1.70.
6	173.98	4.67	1.39	0.25, 0.80 and 1.15.

The bottom velocities were determined by the graphical representation of the velocities observed at the above depths, and the surface velocities were regarded as equivalent to those taken at 0.25 meters below the surface. Since at a gauge reading of 4.55 meters, the breadth of the river was 200.1 meters, the vertical No. 6 was about 26 meters from the right bank.

As the experiments of Humphreys and Abbot, of Darcy and Bazin, and of Dupuit had indicated that the mean velocity in any vertical lay at a distance below the surface equal to 0.577 of the depth, Gerbenau took also a measurement at that point, and compared the observed velocity with the mean velocity deduced from the parabola, and found in general a very good accordance, for in the six verticals the observed velocity was 99.50, 99.85, 103.88, 104.45, 103.04, and 106.07 per cent. of the true computed mean.

The ratio of this mean velocity to the surface velocity varies between 0.7727 and 0.8525, being on the average 0.8226.

An investigation was also made to test the correctness of the formula given by Humphreys and Abbot for determining the mean velocity in any vertical from the greatest and least velocities. On this we shall not dwell as the determination in practice is of no value, since the bottom velocity, the greatest velocity and its position can only be determined by the construction of the parabola.

To determine the water discharge of the Rhine from the mean velocities found in the six verticals at different stages of the river, Gerbenau proposes the following method. He constructs the curve of mean velocity for a guage reading of five feet, divides the section into thirty-nine divisions, computes the area of each division, measures the ordinate of the curve at the center of each division and adds the products of each area by its ordinate. This process gives a discharge of 828.336 cubic meters per second.

In constructing the curve of mean velocity the number of ordinates was increased from six to ten by inserting middle ordinates between the first and second, second and third, etc., verticals. The computation of these middle ordinates depended upon the curve of surface velocities for a gauge reading of five feet and upon the coefficients found in the six verticals for the ratios of the mean sub-surface velocity to surface velocity. Since these coefficients range from 0.77 to 0.85, as above mentioned, the mean of the ratios for two adjacent verticals was used in interpolating the middle ordinate.

We have already intimated that the curve of surface velocities is not so well

determined as we could have wished, and this inaccuracy influences of course the value deduced for the discharge. For practical purposes, however, the results have all desired accuracy. And if the methods followed in finding the mean velocity appear somewhat complex, we must remember that it would be scarcely possible to determine by a shorter process equally exact results from observations made on different days and at different stages of the river.

The mean velocity of the river as thus found was then compared with the maximum velocity, the ratio being 0.7305. The ratio of the mean of all the mean velocities in the six verticals to the mean velocity of the river was also found to be 0.928, agreeing with the value deduced by Humphreys and Abbot.

Besides the measurements of velocity, leveling observations were conducted to determine the slope of the river at the banks and in the thread of the current. These were made in different days and by different engineers, and exhibit somewhat large variations, as shown in the following table:

No.	Gauge Reading.	Fall of River in 24 hours.	Slope on Right Bank.	Slope on Left Bank.	Slope in Center of Stream.
1	5.58 feet.	0.135 meters	0.0011176	0.0009444	....
2	5.00	0.045	0.0011594	0.0010228	....
3	4.90	0.033	0.0012826	0.0013739	....
4	4.35	Stationary.	0.0022527	0.0011821	0.001218

By taking the arithmetical means of these results, Gerbenau concludes that the slope of the river when falling is 0.0011501 and when stationary 0.0012177, and regards it as probable that when rising its slope would be 0.0012853.

It may well be doubted whether this method of determining the slope is entirely satisfactory. The values given are deduced by dividing the difference of level of two points by their distance apart, which on the banks was 200 to 400 meters, and in the current 260 meters. It is evident, however, that the surface of the water for this distance is not a straight line joining the two points whose elevations were measured, but a broken line or a curve. If the slope is to be regarded as a straight line, it should not be deduced from merely the difference of level of the two end points, but intermediate points must be also considered. If the surface is really a curve, the proper slope would be the same as that of a tangent to the curve at its point of crossing the main section.

It may also be questioned whether in such cases the slope should be measured above the section where velocity observations are taken, or at the section itself. This and other questions, such as; how long a distance ought to be taken in measuring the slope; we shall discuss hereafter.

The leveling further showed that the surface of the water at the left bank was from 0.0367 to 0.1680 meters higher than at the right bank, and at the axis of the stream 0.0230 higher than at the left bank. The same peculiarity has also been observed by Siedler in his levels at Basel and by Gerbenau at Maximiliansau on the Rhine. It is evident then, that it is very difficult in rivers like the Rhine to find suitable places for experiments on the uniform motion of water.

Lastly, Gerbenau compares the results of his measurements with those deduced from the formulae of Chezy and Eytelwein, Humphreys and Abbot, Darcy and Bazin, Gaucher, Ganguillet and Kutter. His results were:

Surface breadth $= W = 201.27$ meters.
Greatest depth $= D = 2.785$ "
Wetted perimeter $= P = 202.91$ "
Area of the section $= a = 426.122$ square meters.

Mean radius $= R = \frac{a}{P} = 2.1$ meters.

Slope in axis of stream $= J = 0.0012177$.
Discharge per second $= Q = 828.836$ cubic meters.
Greatest surface velocity $= C = 2.630$ meters per second.
Mean velocity $= v = 1.945$ meters per second.

The formula of Chezy and Eytelwein for the mean velocity is

$$v=50.93\sqrt{RJ}$$

and inserting for R and J the above values it gives $v=2.575$ meters, or 32 per cent. too much. The formula of Humphreys and Abbot,

$$v=7.825496\sqrt{\frac{a}{P+W}\sqrt{J}}$$

gives $v=1.501$ meters or 23 per cent. too little. Darcy and Bazin's formula,

$$v=\sqrt{\frac{R^2J}{0.00028R+0.00035}}$$

gives $v=2.393$ meters, or 23 per cent. too much. Gauckler's formula for slopes of more than 0.0007,

$$v=5.35\sqrt[3]{R}\sqrt[4]{J}$$

gives $v=1.638$ meters, or 16 per cent. too little. Lastly, the formula of Ganguillet and Kutter,

$$v=\sqrt{RJ}\left\{\frac{23+\frac{1}{n}+\frac{0.00155}{J}}{1+\left(23+\frac{0.00155}{J}\right)\frac{n}{\sqrt{R}}}\right\}$$

gives (when n is taken as 0.03) $v=1.9387$ meters, which agrees very closely with the observed mean velocity.

The first of the above formulae appears the poorest, and the last the best in the light of the Basel observations. The agreement in the last case depends, indeed, somewhat upon the lucky choice of the co-efficient n, whose values varies from 0.022 to 0.035.

Besides the main investigations, described above, experiments were also instituted *to compare the accuracy of various hydrometers*, of which a brief account may be interesting.

By comparing the surface velocities as deduced from the observations by Woltmann's mill and the floats, it appeared that the values resulting from the latter were slightly greater. This may, perhaps, be accounted for by supposing that the float which was immersed to a depth of 0.3 meters acquired the velocity of a thread of water at a depth of 0.15 meters, while the mill noted the velocity at a depth of 0.25 meters. Cubic floats 0.3 meters thick, and cylinders 0.1 meters in diameter, gave the same velocities when immersed equal depths. On the other hand, gypsum plates 1.2 meters long, 3 centimeters wide, and 7.5 millimeters thick, gave, when floating on their flat sides, velocities slightly less than their cubic blocks.

These results can only be regarded as approximate on account of the difficulty of determining the exact path traversed by the float. Gerbenau rightly claims however that in taking float measurements the path should be tolerably long and a large number of observations be taken, since in nearly half of his experiments the probable error in noting the time of passage was 0.5 seconds, and in about a quarter of them an entire second.

Unfortunately no experiments with double floats were made, and this is a matter of great importance, since the use of double floats in large rivers can scarcely be avoided while their accuracy is still open to some suspicion.

Woltmann's mill was also compared with two of Pitot's tubes constructed according to Darcy's plan, one manufactured by Bridell and Lanicca, and the other by Salleron. By four observations with the mill, at a depth of 0.25 meters a velocity of 2.1668 meters per second was recorded. By the tube made by Salleron, ten observations showing on the average a difference of head of 34.115 centimeters gave from the formula

$$v=0.84\sqrt{2g(h-h_1)}$$

a velocity of 2.1732 meters, and by the other tube with a mean difference of head of 30.57 centimeters the velocity was 2.0572 meters. The agreement between the mill and the tubes is hence very satisfactory, although the second tube was not properly adjusted.

The differences of head measured by Salleron's tube in the ten observations range from 32.3 to 38.7 centimeters, being 94.7 to 113.4 per cent. of the mean, which correspond to variations of 97.3 to 106.5 per cent. from the mean velocity. This indicates that sufficiently accurate results with the tube can only be obtained by taking a large number of observations.

A second Woltmann's mill was also tried, which had a recording wheel eight to ten centimeters in diameter enclosed

in a cylinder, with four arms curved in a helical form, and the comparison of its results with those of the first mill was most satisfactory. The first instrument made by Ertel gave a velocity less than one per cent. smaller, a deviation not surprising, for the area of its wheel was double that of the second. Gerbenau endeavors to determine from the differences between the single observations the accuracy of these mills, and he shows that the mean error of that made by Ertel is only half as great as for the second and concludes that large mills are to be preferred for experiments in rivers. Although the number of his observations is too small to prove this conclusion, we feel disposed to regard it as correct.

Lastly Gerbenau gives an account of some measurements made by Legler on Nov. 6, 1867, at the upper suspension bridge at Basel 180 meters from the section of the international observations, and with the gauge at 5.7 feet. Only floats were used; there were thin sticks of about nine square centimeters loaded at the lower end so as to float in a vertical position, and thus give the mean velocity of the immersed portion. The path was only thirty meters long, and the floats passed over it in from eight to thirty-one seconds. Observations were made at twenty-nine points in the cross section with usually three floats for each point. For the surface velocities Gypsum floats 1.2 meters long were employed. These as also the sticks appear to have been too light, the latter rarely retaining their vertical position. On drawing the curve of velocities, a very irregular broken line was obtained, such as could not be expected in a cross section so regular and it is hence not surprising that the ratio of the greatest velocity to the mean velocity was entirely different from that deduced by Gerbenau, and that the discharge found was thirty per cent. greater. This shows what great care must be observed in executing such experiments; in particular the floats must have a longer path than was here employed, and precautions must be taken that they preserve their vertical position and reach nearly to the bottom. Even if these sources of errors be avoided, it remains doubtful if such floats actually give the mean velocity in the vertical, and it is much to be wished that this question might be soon decided by careful experiment.

The above extended review will be sufficient to indicate the many interesting problems discussed in Gerbenau's book, and if all have not been solved we are yet thankful that so much light has been thrown upon them, and we can heartily recommend to every engineer the perusal of the work itself.

HARLACHER'S EXPERIMENTS ON THE ELBE.

In the next book which we take up,* Prof. Harlacher, of the Polytechnicum at Prag, gives a short introduction on Hydrography in general, and that of Bohemia in particular, and then discusses at great length the observations made on the Elbe for determining its discharge.

The locality of the experiments was near the boundary of Saxony and Bohemia, about 650 meters below the village Herrnskretschen. At this village a brook joins with the Elbe, and below it at the locality chosen the river is enclosed by retaining walls, and is but slightly curved, its breadth at a normal stage of water being 118 meters, and the greatest depth three meters.

Harlacher used in his measurements a Woltmann's mill, made by Amsler and Laffon, and he gives a beautiful lithographic plate, illustrating the details of its construction. The wheel was sixty-five millimeters in diameter, with two blades fastened to short arms. These blades were of a screw-formed-shape, 44 millimeters long in the direction of a radius, and 90 millimeters wide on the exterior circumference. The arrangement for engaging and disengaging the gear was particularly neat; if not in gear a simple pull on a string brought it into action; if in gear a pull on the same string disengaged it. In many forms of Woltmann's mill, the observer must keep a strong tension on the string to hold it in gear, and such errors are, perhaps, introduced from the action of the water on the string. In Harlacher's machine, the string was enclosed in a tube. Extra arrangements were also introduced to keep the axis of the mill in the direction of the current.

To ascertain the constant of this mill

* A. R. Harlacher. "Beiträge zur Hydrographie des Königreichs Bohmen." Prag, 1872-3-4. Originally printed in the Journal "Technische Blätter."

experiments were made in the still water of Podol harbor. Carried by a boat it was moved at different velocities through the water for a distance of 105.26 meters, the time varying from 41 to 162 seconds, so that the velocity ranged from 0.65 to 2.56 meters, the wheel making from 2.42 to 9.60 revolutions per second. The results of these experiments were represented graphically, the number of revolutions in thirty seconds being taken on the abscissa unit and the corresponding velocity per second as the ordinate unit, the points thus found furnishing a curve, which coincided very closely with a straight line drawn from the origin of co-ordinates. From this curve the velocity corresponding to any number of revolutions in thirty seconds could be seen at a glance, and used in the other graphic computations.

The first measurement was made on April 12, 1871. A gauge was established on the right bank, and the measurement of the velocities and of the cross section carried on simultaneously. As no line could be stretched across the stream, signals were established on shore by which a boat could place itself in the section, where its position was noted by a plane table on shore. Observations were made at nine places in the section, and at several points in each vertical. The first vertical at a distance of 1.4 meters from the left bank afforded only one measurement at a depth of 0.35 meters below the surface; the second at 13.8 meters from the left bank had three at depths of 0.1, 0.8 and 1.5 meters; and so on to the ninth vertical which was 114.57 meters from the left and 5.84 meters from the right bank. It is not mentioned whether each velocity measurement was repeated, and owing to the short time employed in the work, such repetition could hardly be expected.

In considering the working up of these measurements, the graphical method employed deserves particular attention.

If we imagine at each point of a cross section of a river, a line drawn perpendicular to the plane of the section representing the velocity of the thread at that point, we obtain a solid whose volume corresponds to the discharge of the stream. From velocity measurements taken at various points in the section, we may construct this solid, by erecting normals whose length are equal to the observed velocities, and through their extremities passing a surface. The cubic contents of this solid may then be determined by intersecting this solid with horizontal planes, finding the areas of the surfaces thus cut out, and from these areas and the vertical distances between the surfaces computing the volumes of the small prismatic volumes whose sum is that of the solid.

Harlacher applied then the following construction to determine the discharge per second. From the velocity measurements made in each vertical the corresponding velocity parabolas were drawn. Now if at distances of 0.25, 0.50, 0.75, etc. meters, parallels be drawn to the verticals representing the depth of water they will cut the velocity parabolas at the points corresponding to velocities of 0.25, 0.50, 0.75, etc. meters. If all the points at which the same velocity obtains be united, curves will be obtained similar to the contour curves used in computing the contents of hills. The areas of these curves may be measured with the planimeter and used, according to well known methods, in finding the cubic contents.

If these constructions are to be made easily and accurately, observations must have been made at many verticals, and at many points in each vertical. The discharge is, however, much more exactly determined than by using the mean velocity in a vertical as found from the parabola of vertical velocities, and multiplying these mean velocities into the corresponding vertical areas of the cross section.

Harlacher obtained the following as results of his measurements:

Discharge per second = Q = 281 cubic meters.
Area of the section = F = 252.4 square meters.
Mean velocity per second = v = 1.11 meters.
Mean surface velocity = 1.35 meters.
Ratio of mean to greatest velocity = 0.73.
Ratio of mean surface velocity to mean velocity = 1.27.

It is interesting to note that the value of the ratio of the mean velocity to the

greatest surface velocity is the same as that found by Gerbenau.

Besides these measurements the slope of the Elbe was also investigated. From a point on the left bank 114 meters above the section to the section itself, a slope of 0.000395 was found; from the section to a point 95 meters below the slope was 0.000210. On the right (the concave) bank for distances of 76 and 152 meters above the section, the slopes 0.000263 and 0.000395 were found, and from the section to a point 76 meters below the slope 0.000312. The points leveled were plotted and tangents drawn to the point of intersection with the section, from which it was seen that the tangents were parallel although the one on the right bank was three centimeters higher than that on the left. The slope shown by the tangents was 0.000315, while the mean slopes of the upper portion was 0.000399 and of the lower 0.000333.

Prof. Harlacher compares the results of his measurements with those of a great number of formulæ, by inserting his values for slope, area of section, wetted perimeter, etc., in the expressions for mean velocity. The comparison shows that Eytelwein's formula gives a result seventeen per cent. too large, Humphreys and Abbot's seven per cent. too small, Darcy and Bazin's nine per cent. too large, Gauckler's fifty-two per cent. too large, Hageni's eighteen per cent. too large, Bornemann's three per cent. too small and Ganguillet-Kutter's eight per cent. too large. A single experiment like this is, however, not sufficient to decide concerning the generality of a formula, and hence but little value can be attached to this comparison.

The remarks of the author concerning such experiments and the manifold sources of error which affect them are worthy of attentive consideration. As sources of error the following are mentioned: inaccuracy in the measurement of depths particularly in deep rivers with rocky beds; errors in measuring the breadth of the stream, errors in determining the constant of the Woltmann's mill, in fixing the position of its axis, in noting the time, and in throwing the machine out of gear, inaccuracy in reading the number of revolutions, and lastly, disturbance due to impurities in the water. Moreover the accurate determination of the slope is of great influence, in which errors may be committed as high as twenty per cent. of the true value, partly by the agitation of the water level and partly by the sources of error peculiar to leveling. Considering the number and the great influence of these errors, it can scarcely surprise us that no formula has yet been found which can be used in all cases with security.

The above experiments are described in the first part of the book published in 1872. The second part published in 1873 contains an account of more extended experiments to which the preceding may be regarded as a preparation. These were carried on at the same place and in the same general manner, but many improvements were introduced by which more accurate results were obtained. The mill observations were taken from a stage supported by two boats, the time was noted by a watch marking seconds accurately, the positions were located by a theodolite instead of a plane table, the duration of each measurement was sixty instead of thirty seconds, the cross section was determined by four surveys of thirty-seven soundings each, and the Woltmann's mill was adjusted anew.

In finding the slope the greatest care was also taken; the levels extended over 150 meters above and below the section, and points were taken on the water surface at every thirty meters. The heights were plotted on the natural scale, and the distances on a scale of $\frac{1}{1000}$, and through the point where the curve, thus obtained, cut the central section a tangent was drawn as representing the slope. It is remarkable that the slope 0.000315 was found for high, mean and low water stages, the same value as determined in the survey of the previous year.

During the first observations the water surface stood at 2.05 meters below an assumed zero point of reference, and nearly the same as in the survey of 1871; the discharge per second was found 270 instead of 281 cubic meters. During the second set of observations the surface was 2.49 and in the third 3.06 meters below the zero point.

The velocities observed at different points in the verticals, were used in con-

structing the so-called parabolas of vertical velocities, nine of which were drawn from the first set of observations, ten from the second, and nine again from the third. These curves showed that the greater velocity in a vertical was sometimes at the surface and sometimes below the surface; and that it appeared to be lower as the stage of water diminished; further, that the mean velocity of a single vertical obtained by dividing the area of the parabola by the depth of water was at mean high water from 0.58 to 0.67, at mean water 0.50 to 0.67, and at low water from 0.54 to 0.62 of the depth below the surface; and, lastly, that the bottom velocity was from 19 to 60 per cent. of the surface velocity, or from 22 to 76 per cent. of the mean velocity.

These relations are readily exhibited to the eye by the graphical constructions, which give the three profiles to an exaggerated scale, and the velocity curves. The curves of mean velocities in the single verticals, of maximum velocities and of mean velocities in the whole stream are drawn upon the cross-section, with also curves added showing the surface and the bottom velocities. Three tables are given in which the relations between these quantities are exhibited numerically, and from which we make the following extract:

	High Water.	Mean Water.	Low Water.
Area of section in square meters	245.40	192.00	119.30
Mean velocity in meters per second	1.10	0.95	0.76
Discharge in cubic meters per second	270.00	182.30	90.80
Greatest velocity in the section	1.48	1.30	1.12
Mean surface velocity	1.27	1.10	0.85
Greatest surface velocity	1.42	1.27	1.11
Mean bottom velocity	0.57	0.44	0.38
Ratio of mean velocity to maximum velocity	0.74	0.73	0.68
Ratio of mean velocity to mean surface velocity	0.87	0.86	0.90
Ratio of mean velocity to mean bottom velocity	1.93	2.16	2.00
Ratio of mean velocity to greatest surface velocity	0.77	0.75	0.69
Ratio of mean velocity to mean depth	0.55	0.59	0.67

This table is very valuable, showing as it does that the different velocities are not related to each other in a constant manner, and also that, for the depths at which they are found no general formula can be obtained.

If now it be required from these three measurements to find the discharge at other stages of water, it can within certain limits be obtained by an easy graphical construction. As, however, the area of the section for different stages can be quite accurately determined and the discharge is the product of the area by the mean velocity, Herlacher undertakes to find an interpolation formula for the latter, none of the formulæ thus far published being regarded as sufficiently accurate. He proposes that the mean velocity be regarded as a function of an unknown power of the mean radius and the slope, and seeks to calculate this power from his observations which offers but little difficulty since the slope at all stages of water was found the same.

Accordingly in the equation

$$v = \beta r^x$$

only the coefficient β and the exponent x are to be determined, and from the three observations,

$v=0.76$ and $r=1.13$ at low water

$v=0.95$ and $r=1.60$ at mean water

$v=1.10$ and $r=2.01$ at low water

he finds for x the corresponding values, 0.642, 0.642 and 0.643, and for β the values 0.7026, 0.7026, 0.7026, so that the formula becomes

$$v=0.7026\ r^{0.642}$$

A comparison of the results computed from this and other formulae shows that at low water many of them give nearly the same values, but that at high water the differences are quite marked. The curve computed from the above expression for v lies between those given by the formulae of Darcy and Bazin and of Ganguillet and Kutter, as also between those from the formulae of Humphreys

and Abbot and of Gerbenau, is, in fact, nearly a mean between them. Nevertheless, the new formula can only be accepted with circumspection since the assumption that the fall of the river at all stages of water is the same, as found by the Elbe observations, can scarcely be entertained without further experimental proof. It is possible, indeed, that at the place of observation on the Elbe, the slope would have been found different from 0.000315 for very high or very low stages of water.

As already remarked, Herlacher deduced the above formula only for the purpose of computing the discharge through the section at Herrnskretschen for different stages of water. For this computation continuous readings by a self registering gauge would have been much better. As none, however, were taken, the gauge observation at Tetschen and Schandau were used to find by interpolation the simultaneous stage of water at the intermediate measuring station. For this a series of corresponding gauge readings at Herrnskretschen, as well as at Tetschen and Schandau was of course necessary since the water level at these three points does not equally rise and fall. The interpolation was made graphically by laying off the water stage at the section as ordinate, and that at Tetschen or Schandau as abscissas, and connecting the corresponding points from which a curve representing the variation of water at the section for the last six months of 1871 was obtained. From this it was seen that the mean reading (below the assumed zero point) was 2.84 meters, while that at Tetschen was 0.33 and at Schandau 1.22 meters. The highest water which occurred in July 8, was 1.47 meters above the mean, the lowest on Dec. 7 was 0.68 meters below.

For each stage of water the mean velocity can now be computed from the formula

$$v = 0.7026 r^{0.642},$$

since the corresponding cross section can be constructed and the mean radius $r = \frac{F}{p}$ easily found. The discharge is then simply $Q = Fv$. Representing the various values of Q by ordinates to a straight line where abscissas are the times, a curve is obtained whose areas represent the discharge for any required time intervals.

Such a curve was constructed by Harlacher and the areas measured by the planimeter. He found the discharge to be

In July	639,317,000 cubic meters.
August	359,917,000
September	196,727,000
October	305,230,000
November	221,391,000
December	334,459,000

The mean discharge is then 11 million cubic meters daily, or 130 per second. The least daily discharge 3689000 on Dec. 7, and the greatest 36858000 on July 8.

Taking the monthly discharges and determining from them the stage of water, we obtain nearly the same values as by taking the arithmetical mean of all the observed stages; thus:

	July.	August.	September	October.	November	December.
Mean discharge per second.	238.7	134.4	75.9	114.0	85.4	124.9
Corresponding water stages	−2.20	−2.27	−3.18	−2.90	−3.11	−2.83
Observed mean stage.....	−2.22	−2.77	−3.18	−2.91	−3.11	−2.87

The sign—prefixed to the water stage shows that the surface of the water lay below the zero point of reference.

Here closes the second part of Harlacher's contribution to the Hydrography of Bohemia, a work distinguished for its elegant graphical constructions, and which may be well taken as a pattern in the execution of hydrometric experiments. Could such experiments be made on all our great streams an interest in technical circles, equal to that excited by the measurement of the arc of a meridian in Central Europe, might be awakened, and as so much time and money have been spent on the latter, it is possible that governments might be induced to furnish the necessary means for the execution of comprehensive hydrographic

surveys, if engineers would only take the initiative in proposing it.

In the third part of his work Herlacher treats of the discharge of the Elbe in the first six months of 1872, according to the same plan as before. He regards it as well to begin the hydrographic year with July, and to end it with the following June, since the influence of the winter snows is the better included.

Since in this period no new measurements were undertaken, the discharge was computed on the supposition that the slope of the river remained 0.000315 even at higher water stages (which renders the results somewhat questionable) by finding the mean velocity from the formula

$$v = 0.7026 \left(\frac{F}{p}\right)^{0.642}$$

The results are given in the following table :

	January.	February.	March.	April.	May.	June.
Mean discharge per second.	190.	166.	343.	299.	348.	221.
Corresponding water stages.	−2.45	−2.58	−1.70	−1.90	−1.68	−2.29
Observed mean stage.....	−2.46	−2.62	−1.77	−1.93	−1.91	−2.35

We see here again that the mean of all the observed water stages does not exactly correspond with the stage of water as deduced from the mean discharge. For the entire half year the discharge is 262 cubic meters per second, or nearly double of that of the preceding half year. The differences between the smallest (43 and 113) discharges and the greatest (429 and 2485), are still more striking.

Considering this discharge with reference to the water shed of the Elbe above the section of Herrnskretchen, which has an area of 50,600 square kilometers, it appears that in the second half of 1871 there were discharged 40,600, and in the first half of 1872, 81,500 cubic meters per square kilometer, which, for the whole year, from July, 1871, to June, 1872, is only 25 per cent. of the rainfall.

Particularly interesting in this third part is the account of a sudden flood in the river, arising from a local storm near Beraun in May, 1872, in which gauge readings at 38 stations, over a distance of 836 kilometers, were taken. In this investigation, not merely the actual gauge readings, but the differences between the readings during the flood and those immediately before, are properly discussed. The curves which represent these changes at 24 stations between Prag and Hamburg, from May 25 to June 8, 1872, give a clear picture of the course of the high water, arising from a local rain near the head of the river. It decreases in height steadily as it descends the stream, while its width increases; in the upper river it rises quickly and remains but a short time at its maximum height, in the lower, however, rising slower and lasting longer,—exhibiting the characteristics of a great wave; even in Hamburg its influence was perceptible on the gauge readings. The 215 kilometers from Prag to Dresden were passed over in 34 hours, the 378 kilometers from Prag to Wittenberg in 88 hours, and the 794 kilometers from Prag to Hamburg in 240 hours, showing that its velocity decreased as it neared the sea.

The high water arising from the melting of the winter snows flows somewhat differently, since it receives from all sides constant accessions. We should here expect that the high water would continually increase as it descends the stream. This is not exactly so, as Harlacher shows, for the form of the river bed exerts considerable influence. In the Elbe the spring floods increased in depth only to Tetschen, below which they again decreased. The summer floods appear to follow a slightly different law, as is seen from a single example discussed.

GORDON'S EXPERIMENTS ON THE IRRAWADDY.

The works noticed above are of a decidedly practical character touching the theoretical side of the subject only in connection with the measurements made by their authors. In the two brochures of Mr. Gordon* we have, however, a

* Robert Gordon. "Fragment containing a discussion of a new Formula for the flow of water in open channels." Milan, 1873.

theoretical decision of the laws of flowing water, his extensive hydraulic observations being only incidentally mentioned. The first which he moderately calls a "Fragment" was printed for private circulation, and comes before us with no pretensions to offering anything new even in theoretical hydraulics.

It opens with a justifiable complaint concerning the numerous formulae for the motion of water in rivers, formulae containing so many different constants raised to so many different powers and leading to results so widely varying, all of which seem to be only empirical and which are hence changed by every new series of experiments. It then investigates the reason of the failure of previous efforts to establish a general valid formula, points out that this lies not so much in the imperfections of the observations as in the inaccuracy of the fundamental theoretical conceptions, subjects these conceptions to a searching examination and closes with a proposed new formula.

All the theories on the subject hitherto set up appear to the author to be founded on the three conceptions of Castelli, Torricelli and Dubuat. In the beginning of the seventeenth century Castelli announced that the velocity of the water in a river would be doubled if its depth were doubled; that the velocity of a thread of water increases proportionally to the pressure or head so that the so called vertical velocity scale is a triangle whose vertex is at the surface and base at the bottom of the river. This last was modified by one of his pupils, Cavaliere, by considerig that the upper layers of water must be carried along with the lower ones so that the velocity scale would be the triangle reversed.

Torricelli demolished Castelli's hypothesis by carefully conducted experiments, and showed that here the same law exists as in the free fall of a body. Starting with this more correct conception, Guglielmini originated the first actual theory of the motion of water in rivers. He said, that in this motion the same law prevailed as in the descent of a body on an inclined plane. On account of the mobility of the single particles of water a river varies in cross section as its velocity changes. The acceleration or retardation of the single molecules is communicated to neighboring molecules, but in diminished proportion with the distance. The resistance of the bottom destroys the acceleration due to the inclination. If the motion is permanent the stream retains the velocity previously acquired, which is the greater the greater the slope. On account of the variation in the resistance, permanent motion seldom is found in streams; rivers with a bed of boulders are in a continually changing condition of acceleration and retardation. Since the upper layers press upon the lower ones, the motion of the stream depends not merely on the slope, but also on this pressure; near the head of the river where the slope is large the velocity is due mostly to the slope, lower where the slope is small the head of water enters as a disturbing cause, and in every section of the stream the velocity of the lower layer will be determined more by the head, and that of the upper layers more by the slope.

In consequence of the influence of the pressure, the velocities in a vertical would follow a parabolic scale; this can rarely be true in fact, since the resistance at the bottom is much greater than at the surface.

These views found acceptance till the time of Dubuat, although Couplet's experiments in the water pipe at Versailles showed that it was impossible that the velocity could increase as the square root of the head. Dubuat, however, stated the principle, which to-day is regarded correct, that the surface slope is to be considered as the only cause of the motion of water in rivers. This leads to the conclusion that if the influence of the resistance were absent all the molecules in a section must have the same velocity, since the motion of a single molecule depends only on the difference of pressure, exerted by the preceding and following molecules, and since this difference is equal for all points of the section.

Gordon regarded the theory of Guglielmini as preferable to that of Dubuat, and he endeavors in his Fragment to demonstrate this, and to deduce a new formula for the motion of water in rivers, having previously given a critical review of the various formulæ hitherto proposed.

[1] Robert Gordon. "On the Theory of the flow of water in open channels." Rangoon, 1875.

As these formulæ have but little more than an empirical value, we shall not dwell upon Gordon's discussion.

In deducing the new formula the method followed is long and not sufficiently clear, and we pass it over to give the result obtained, noticing that the author confesses at the end of his investigation that it does not correspond to experimental results, but that one member must be divided by the $\frac{3}{2}$ power of the slope to make an accordance. It will be sufficient to give his formula and the experimental verification. The formula is:

$$PWD=\left(\alpha+\frac{\beta W\rho^2}{R^2\sqrt{D}}\right)p\rho V^2$$

in which the letters have the following significations:

P=depth at which the center of pressure of the section lies.
W=breadth of stream.
D=slope.
R=mean radius.
p=wetted perimeter.
V=mean velocity.
ρ=coefficient of roughness.
α and β are constants to be determined.

Gordon then gives a plate in which five series of Bazin's experiments, viz., those on canals lined with smooth cement, with boards, with bricks, with small and with large pebbles, are represented according to Bazin's corresponding five formulæ and according to the new formula, the values of α and β being taken at $\alpha=0.000112$ and $\beta=0.00000007$, while according to the degree of roughness, ρ takes the values 1.00, 1.36, 1.47, 2.46 and 3.30. In Bazin's experiments the constant slope 0.0049 was employed; Gordon, however, gives seven series of experiments with board canals having slopes of 0.00824 and 0.00208, and these, as also the former, agreed closely with the new formula.

Gordon also compares the results of his experiments at Irrawaddy, which we shall describe below, with his new formula, and finds a satisfactory agreement when the coefficient of roughness ρ is placed at 2.23. The following table contains in *English measures* the principal data of these experiments:

Date in 1872.	Gauge Reading.	Daily rise (+) or fall (−).	Slope multiplied by 10,000.	Cross Section A.	Discharge Q.	Mean Velocity V.	Wetted Perimeter p.	Width at Surface W.
Aug. 20	36.00	−.	0.947	223,516	1,442,007	6.451	5220	5020
" 23	33.75	+0.25	0.933	212,403	1,212,190	5.706	5215	5008
Oct. 18	32.25	+1.09	0.925	204,988	1,148,790	5.604	5212	5002
Sept. 9	31.83	−0.50	0.925	202,921	1,098,288	5.365	5208	5000
" 29	29.75	+1.00	0.918	193,083	985,959	5.106	5205	4992
" 21	26.58	+0.92	0.911	177,123	871,823	4.922	5196	4990
Nov. 4	24.08	+1.00	0.905	164,633	757,132	4.599	5191	4975
Oct. 30	23.91	−1.25	0.905	163,313	712,125	4.360	5190	4974
Nov. 2	22.08	−0.25	0.898	154,750	667,268	4.228	5185	4968
" 8	22.08	−1.08	0.898	154,750	627,925	4.057	5185	4968
" 12	17.75	−1.17	0.884	133,363	483,477	3.626	5160	4942
" 14	15.75	−1.00	0.887	123,483	416,139	3.370	5150	4928
" 16	14.33	−0.75	0.871	116,649	370,072	3.258	5140	4915
" 21	12.08	−0.42	0.864	105,353	307,256	2,767	5020	4770

In the following table the last column is added by the reviewer, and shows that $\frac{PWD}{p\rho V^2}$ is very well represented by $\left(\alpha+\beta\frac{W\rho^2}{R^2\sqrt{D}}\right)\rho$ when the values $\alpha=0.0000774$, $\beta=0.00000012$, $\rho=2.23$, are given to the coefficients. Other values for these coefficients than those found by Bazin for his small canal are hence necessary, although it is not to be denied that the formula agrees very well with the experiments. The table is given in *French measures*, the values of p, W and V being repeated:

Date in 1872.	p	W.	R.	P.	V.	$\frac{PWD}{pV^2}$	$\frac{W}{R^2\sqrt{D}}$	Computed value of $\frac{PWD}{pV^2}$
Aug. 20	1591	1530	13.047	11.098	1.965	0.000262	923	0.000288
" 23	1589	1526	12.410	10.428	1.739	309	1026	303
Oct. 18	1588	1524	11.984	10.197	1.708	309	1103	313
Sept. 9	1588	1523	11.872	10.117	1.635	334	1124	316
" 29	1585	1521	11.303	9.872	1.556	354	1242	331
" 21	1583	1520	10.386	9.217	1.500	346	1476	363
Nov. 4	1582	1516	9.663	8.821	1.401	378	1707	393
Oct. 30	1581	1516	9.588	8.792	1.329	413	1733	397
Nov. 2	1580	1514	9.094	8.532	1.288	426	1932	423
" 8	1580	1514	9.094	7.861	1.236	438	1932	423
" 12	1572	1506	7.875	7.589	1.105	509	2582	510
" 14	1569	1502	7.034	7.589	1.027	560	3012	567
" 16	1566	1498	6.915	7.359	0.993	567	3356	623
" 21	1530	1460	6.393	7.039	0.843	0.000731	3995	0.000698

Gordon gives also the following account of these extensive experiments. They were undertaken to determine the loss of water by leakage through the dykes which had been built for a great length in the delta on the right bank of the Irrawaddy River. At the head of the delta, near Saiktha, and also 110 miles below, near Zaloon, measurements were made in the same manner as those of Humphreys and Abbot on the Mississippi; viz. surface velocities were observed about sixty times a day by double floats, whose lower point was one meter below the surface. Ten series of observations were also made daily on velocities at greater depths, the position of the lower float being placed one meter deeper each time till it touched the bottom. The arithmetical mean of these velocities was taken, and the ratio between this mean and the mean surface velocity in the same vertical was found, by whose help the mean velocity in a vertical for each observed surface velocity was found. The river which, at Saiktha, is over 1,500 meters wide, and at high water 23 meters deep, was divided into ten divisions, and the discharge was found by multiplying the area of each division by the mean of the mean velocities of the verticals contained in that division.

The determination of the slope was particularly difficult. From the levels extending over about eighteen miriameters in length a slope at high water of six inches per mile (0.000095) was found. At Myanoung (22.5 kilometers from Saiktha and 320 kilometers from the ocean), the high water level lies 23.16 meters above the mean sea level and the low water level 10.97 meters, giving a variation of 12.19 meters. At Saiktha from high to low water there is 12.8 meters, at Henzadah (120 kilometers south of Myanoung) 11 meters, and at Zaloon 10.5 meters difference in level. The different stages of water, hence change the above given slope considerably; the difficulty is also increased by the sandy bottom of the river which retards the flow at low water. At high water the stream takes moreover a shorter course than at low, so that the slope at low water is reduced to four and two inches per mile.

Under these difficulties it was scarcely possible to find the slope corresponding to each observation, and Gordon thinks that only for the gauge readings from 14 to 36 feet was it determined for a distance of 16 kilometers with sufficient accuracy.

For the computation of the depth P at which the center of pressure of the section was located the formula

$$P = \frac{\int x^2 y dx}{\int x y dx}$$

was used, in which y is the breadth for any depth x and dx is an increment of two feet.

From these experiments, which are not further described in Gordon's pamphlet, it is only possible for him to explain why it happens that the formulæ based upon Dubuat's theory give in general extreme-

ly satisfactory results. According to his views, this lies in the fact that the values of P and R in many cases are very nearly equal, and he gives as proof the following data from the Irrawaddy survey:

Gauge reading=	36.0,	29.8,	24.1,	17.7,	12.1,	6.0,	1.9.
R =	43.6,	38 0,	32.3,	26.4,	21.3,	20.2,	19 3.
P =	35.7,	32.4,	29.0,	25.8,	23.1,	20.1,	18.2.

Since the numerical values of P and R are so nearly equal, the numerical values of the quantities $\frac{PWD}{pV^3}$ and $\frac{RD}{V^3}$ come also nearly equal, and in these quantities lies the characteristic difference between the new formula and the old ones.

The second work whose title we have given above is a supplement to the preceding, containing more complete information concerning the extensive hydraulic measurements, whose results had incited Gordon to deduce a new formula for the motion of water in rivers.

As already mentioned the experiments were conducted on the same plan as those of Humphreys and Abbot. The Mississippi observations however were confined to observations in 254 verticals, while those at Irrawaddy were carried on continually from Aug. 1, 1872, to Sept. 1, 1873, (Sundays excepted), so that velocity curves for 7000 verticals were obtained.

For the velocity observations the so-called double float was used. This consisted of a light wooden disk, 25 millimeters in thickness, and 152 millimeters in diameter, which floated on the surface, and to which was attached by a cord the lower float—a wooden cylinder 305 millimeters long, and 152 thick, so loaded that the upper disk remained only 6 millimeters above the water surface. The cord was 1.6 millimeters in thickness and was varnished, its length being so arranged that the lower float was always either 1, or 2, or 3, etc., meters below the surface; the velocities at these depths were observed in succession, and the irregular movement and frequent immersion of the upper one showed when the bottom was reached, although a greater velocity was here often observed (remarkable to relate) than at less depths. These floats were numbered and were, as ordered, dropped in regular succession from an anchored boat lying far above the place of observation, their passage across two established lines (60.95 meters apart) being noted by theodolites and chronometers on shore. Velocity curves for 10 verticals were daily thus determined, and by 30 to 60 measurements at a depth of 1 meter, a curve of surface velocities also daily established. Any change among the observers was carefully avoided, and exact compliance with the instructions was required. The observers, moreover, were unacquainted with the theoretical aim of the investigation. During the time of observation there was one low and two high water periods. The principal section was near Saiktha at the head of the delta; and since the number of measurements was too great to allow of immediate reduction only those made in 3000 verticals at Saiktha were subject to computation.

From the velocity measurements made in the verticals at every meter in depth the vertical velocity curves were constructed, and a comparison of the curves for different stages of water affords a view of the changes produced by changes in the level of water. The irregularities disappear when many curves under similar conditions of gauge reading, depth of water and distance from the bank, are combined. Hence Gordon selects a limited number of such curves for each division of the cross-section, and combines them to obtain a mean curve. This was done for different stages of water and thus about 2000 curves were found which, in Feb. 1874, were communicated to Bazin, who still further studied them and combined all those found at low water, as also those for high water. In this way 500 reduced curves resulted, which represent indeed a wonderful industry.

In the Irrawaddy River the guage reading at the highest water is 40 feet, but in the years 1872 and 1873 it only reached 36 feet. The lowest known low water is 2 feet below zero, but in 1873 it did not quite reach this limit. Bazin hence considered two low water stages, from 0 to 3 feet, and from 3 to 5 feet, as also two high water stages from 22 to 30, and from 30 to 36 feet gauge reading. He divided the section for low water into four, and for high water

into six divisions and grouped all observations of verticals of equal depth in a division together, viz., from 17 feet in depth to 70 feet. Since, however, the absolute distances did not allow a strict comparison, he divided the resulting mean depths for each group into ten equal parts and computed the corresponding velocities and from their values constructed the vertical velocity curve for each vertical. From these mean vertical velocity curves for both low and high water stages were found, so that finally only four curves remained, in which, of course, the individualities of the original curves have disappeared. In the book the complete numerical data are given in order that any one may construct the original curves for himself; we think it, however, a pity that they were not worked up according to Prof. Harlacher's method.

By comparing this enormous number of vertical velocity curves, Gordon finds (and it is also easily seen from the figures in his work) that for a constant gauge reading the ratio of the velocity at any depth to the surface velocity increases as the depth of the vertical increases; and that for the same point of the cross section. This ratio is considerably greater at high than at low water. While at low water the surface velocity is always perceptibly greater than the velocity below the surface (excepting, perhaps, in the deepest places), the difference of the velocities in different depths of the same vertical decreases more and more as the water rises, until in the greatest depths of sixty to eighty feet the greatest velocity occurs near the bottom of the river. This remarkable phenomenon was established not only in the section at Saiktha, but also in a section 140 kilometers below.

It may now be asked whether the phenomenon here brought to light is to be regarded as a universal law as Gordon supposes, or whether it was only produced by some local influence which modified the law as given by the parabola of vertical velocities. The sand banks in the Irrawaddy, which are so numerous as to be dangerous to navigation, may be regarded as such a local cause, even if the great distance (sixteen miles) of the nearest bank below Saiktha would seem to forbid such assumption. The slope of the river is indeed extremely small (0.000095) so that a bank of considerable height, which would be dangerous to ships drawing 1.2 meters, would be perceptibly higher than the bottom of the river at Saiktha. Unfortunately, a satisfactory view of the question is not to be had, because thorough levels of the river bottom and surface are not communicated; and hence the results of Gordon's extensive experiments are only to be accepted with great circumspection. Special levels were here, if any where, certainly necessary, in order to render useful experiments carried on with so great energy. It is also to be wished that a longer path for the floats had been employed, and that the number of floats used for the determination of the surface velocity (30 to 60 on a width of 1000 to 2130 meters), as also the number of verticals for velocities below the surface (10) had been greater, particularly in the wider places. Gordon's experiments, however, inspire a far greater confidence than those of Humphreys and Abbot, for Hagen has shown that it is extremely probable that their results were altered in part to establish the theory proposed. His velocity measurements are also free from the suspicion attaching to theirs, for his lower float was connected with the upper by a very small cord, while in the Mississippi Survey the surface of the cord for measurements at a depth of 100 feet was $1\frac{1}{2}$ times as great as that of the float itself. Against the use of double floats there is little to be objected, since the comparative observations at Basel show them to be not untrustworthy. Other experimenters have also observed the phenomenon that the velocity decreases below the surface without being able to point out a sufficient cause for its occurrence. Although the new theory needs further tests, Gordon's observations are worthy of great attention, and his publications are certainly to be reckoned with the most interesting of late times in this department of literature.

REVIEW AND COMPARISON.

Arrived at the end of our report, we allow ourselves to take a backward glance at those points which need to be particularly observed in future hydrometric experiments. Whenever possible measure-

ments of velocity should only be made with a carefully adjusted Woltmann's mill of suitable size, and each measurement should be at least once repeated, the time never being less than one minute. In wide and deep rivers where the mill cannot be used the so-called double float should be employed. The size of the float should correspond to that of the stream, and if possible ought to be spherical and so loaded that the cord should not be too slack; the accuracy of this instrument is, however, not yet satisfactorily proved. Particularly important is the length of the path over which the float passes; it should be long enough so that the time of passage of the greatest velocity may be about one minute. The course of the float ought to be observed, and such rejected as deviate from a straight line parallel to the current. Lastly, it is advisable to allow many floats to pass at each point, in order to obtain the mean time of passage.

It is self evident that a hydraulic survey will be the more accurate, according as more simultaneous velocity measurements are taken at different points of the cross section.

In working up the observations, the method proposed by Prof. Harlacher yields the most comprehensive and the most accurate results: if, however, the experiments are carried on at different times, Gerbenau's method of interpolation is to be recommended. Graphical representations prevent gross errors, and permit all the relations to be more clearly seen than in mere numerical work.

If such experiments are to be used in investigating the law of the flow of water in rivers, as great care must be taken in the choice of the places for observation, and in the determination of the slope and the cross section, as in the execution of the velocity measurements. Unfortunately a considerable diversity of opinion exists as to what should be regarded as the slope, and it may be hence recommended to make accurate levelings on both banks, and in the current for a length of five times the breadth of the stream and to draw them on profile paper, from which the total slope as also the local slope in the cross section may be easily seen.

Considering, however, that in general, in great rivers especially, a permanent motion of the water is scarcely to be expected, it appears more correct to carry on such experiments in the following manner, pointed out several years ago by Weisbach:

Let two cross sections be taken at a sufficient distance apart, and let their areas F_0 and F_1, their wetted perimeters p_0 and p_1, and the mean velocities v_0 and v_1 be determined, as also the total fall h and the distance l between them. Then from the relation

$$h=\frac{v_1^2-v_0^2}{2g}+z\frac{l}{2}\left(\frac{p_0 v_0^2}{2gF_0}+\frac{p_1 v_1^2}{2gF_1}\right)$$

the coefficient of resistance z may be found. The greater labor arising in this method may perhaps be out-weighed by the greater value of the results obtained. The two sections, which under a permanent motion of the water will give equal discharges, furnish a proof whether the place chosen is suitable for such experiments, while the uncertainty in regard to what is to be considered the proper slope disappears.

THE RESISTANCE OF VESSELS.

From "Engineering."

Of all the abstruse subjects connected with naval architecture, that which has hitherto been most involved in mystery, and has most successfully defied all attempts to unravel its subtle intricacies, is unquestionably the subject of resistances. Marine propulsion in most of its branches has indeed, until within the last few years, been almost a sealed book even to the most able investigators; and the absence of definite knowledge and of exact reasoning based upon sound principles and correct data, has naturally favored the growth of erroneous doctrines and given a fictitious importance to formulæ equally erroneous in princi-

ple, although offering at least some assistance to the naval architect. The old Admiralty formula

$$S'=\frac{C\times IHP}{D^{\frac{2}{3}}} \text{ and } S'=\frac{C_1\times IHP}{A}$$

the more modern formula of Professor Rankine, and the various theories on resistance put forward by Mr. Scott Russell, and many others advanced from time to time, are all doomed to vanish before the light that Mr. Froude is gradually throwing on the subject. He conquers the subject a bit at a time, but his progress is sure, and each fresh step, while it shows how far abroad everybody has been up to the present, shows also more clearly than ever that Mr. Froude is on the right track, and encourages us to hope that he will soon reach a complete and satisfactory solution of the puzzling phenomena which surround the movements of a ship through the water.

An enormous stride was made when Mr. Froude discovered the relation which exists between the resistance of ships and their models. Having found the "corresponding speeds" at which the resistance of ships and their models would be proportional to their displacements, a new era was commenced, because for the first time it became possible to argue safely from the results of experiments with models to the performances of ships. Following this up he has shown that the area of midship section, as such, plays but a very unimportant part in the resistance of ships instead of occupying as it has hitherto done the most prominent position. The idea that as the area of midship section measures the area of the canal that the ship digs, as it were, for herself through the water, it must be the chief measure of her resistance, is certainly a most plausible and taking one, and although Mr. Froude has shown it to be utterly fallacious, yet we doubt not it will be clung to by many for some time to come. The point to which Mr. Froude has dovoted himself this year at the meetings of the Institution of Naval Architects is the effect on the resistance of adding to the length of straight middle-body in ships. And the results are most valuable and instructive, for they throw an entirely new light on the subject of resistances, and account for many strange results that have appeared anomalous and perplexing, whether looked at from a practical or scientific point of view. It was for a long time thought that adding to the length of the straight middle-body could not affect the resistance. Since the important part played by skin friction in the resistance of ships became recognized, however, it has been conceded that the effect of adding to the length of the straight middle-body would be to materially increase the resistance, owing to the increased resistances due to skin friction, and it has been shown very conclusively that better results might be obtained under some circumstances, by a somewhat broader ship with finer ends of the same displacement, and having no straight middle-body. There can be little doubt that a great bar to the acceptance of these ideas by shipowners and shipbuilders has existed, owing to the very favorable results found to arise, within the last few years, in not one ship or two ships, or a dozen ships, but in scores of steamers, by cutting them amidships and lengthening them by the addition of a piece of straight middle body. We have heard instance after instance quoted where the vessel has carried more cargo, steamed faster, and burnt less coal after being lengthened, and in fact vessels have been converted from unprofitable to profitable ships by the simple process of lengthening. Where great improvements have been made in the engines and boilers at the time of lengthening the ship, such as when they have been "compounded," it would be only natural to expect better results than formerly, and to combine less coal consumption with greater cargo capacity. Where little or no change had been made in the machinery, and yet coal consumption and speed were said to have remained as well as before, or even better, with a greater skin resistance and greater weight of cargo—and we have often heard such facts asserted firmly by people in a position to know the truth—the question became not a little perplexing, especially in view of other somewhat similar cases of lengthening in which a loss of speed of nearly half a knot had been the result.

Mr. Froude's paper supplies a curious and valuable explanation of such apparent anomalies. His experiments were made with models of ships varying in

length from 180 feet to 500 feet long, all having the same beam 38.4 feet, the same draught 14.4 feet, the same form of midship section, and the same length of fore-body and after-body, or entrance and run, viz., 80 feet each. The shortest ship had no middle-body, and the longest had 340 feet length of straight middle-body. The intermediate vessels went in steps of about 20 feet of middle-body, everything else remaining the same; in fact the shorter models were made from the longer, by cutting out successive lengths of middle-body and rejoining, so as to keep the same surface friction. The displacements of the ships represented by the models ranged from 1,245 tons to about 6,000 tons by intervals of about 142 tons.

The resistance of each model at different speeds was ascertained, and the corresponding resistances of the series of ships were set up in the usual form of "curves of resistances." These curves proved to be by no means fair curves, such as one would expect on the supposition that the resistance varied as the square or any other power of the speed, but presented undulations—"humps or contrary flexures." And instead of the resistance in each case going up regularly as the middle-body was lengthened, a curious alternation of excesses and diminutions of resistance was observable.

"Comparing together the curves of resistance of these ships, we find that at the lower speeds every added 40 feet of length (and 142 tons of displacement) increases the resistance by about the same amount; but at the higher speeds this harmony disappears. At 13 knots, for example, the 200-feet ship makes considerably more resistance than the 240-feet ship, which has 568 tons more displacement; and though at 14½ knots the longer ship again makes the greater resistance, yet even at 14 knots the 280-feet ship makes less resistance than both the 200-feet ship of 1,137 tons less displacement, and than the 240-ton ship of 568 tons less displacement; and at 14½ knots the 200-feet ship makes almost as much resistance as the 360-feet ship of 2,275 tons more displacement. Similar anomalies appear in the comparison between other ships. The tendency to alternate excesses and defects of resistance in the shorter ship as compared with the longer appears throughout the diagram."

Mr. Froude then proceeds to analyse these results, regarding the resistance of a ship as made up of three items, viz., skin friction, eddy-making resistance, and wave-making resistance. The former is approximately proportional to the area of skin, so that addition of successive equal increments of parallel side can only affect it to the extent of producing corresponding equal increments for every additional length. "The anomalies we have noticed," says Mr. Froude, "can only be the result of some unexpected effect which the distance between the two ends produces upon the other two items which make up what may be conveniently termed the 'residuary resistance.'" Mr. Froude then eliminates the known fractional resistances from the total resistances given on the curves, and makes a fresh series of curves, the abscissae being the lengths of straight middle-body for the respective ship and the ordinates being the "residuary resistances" for the corresponding ship at the several rates of speed. A series of curves are thus formed, one above the other, for different speeds, each curve representing the gradual change in "residuary resistance" corresponding to the gradual elongation of the middle-body at a particular speed. From these curves it is seen "that up to a speed of about eleven knots they are straight and level, showing that the residuary resistance is practically unchanged by insertion of parallel side; but that at higher speeds they present a series of regular undulations, showing that the gradual insertion of parallel side produces an alternate increase and diminution in the residuary resistance."

These undulations in the curves of residuary resistance explain and harmonize with the apparent anomalies in the comparison of the curves of resistance before referred to, but how are they to be accounted for? This question has fortunately not been left unanswered by Mr. Froude. By carefully observing the waves formed along the side of the model corresponding to the 500 feet ship, when running at a speed corresponding to 14.3 knots, the positions of the crests and hollows were ascertained,

and it was found that a close connection existed between the positions of those crests and hollows, and the positions of maxima and minima formed by the undulations on the curves of residuary resistance. It became, in fact, clear "that the residuary resistance is smallest when the middle-body is of such a length as to place the middle point of the after-body where a wave crest would be if the middle-body were continued, and largest when of such length as to place it where a trough would be. The inference is, that according as a wave crest or a wave hollow comes about the middle of the after-body so will the forward pressures of the water on the after-body be greater or less, and the total resistance of the ship be less or greater. In the very long parallel-sided form, 500 feet, the sternmost of the train of waves left by the bow had become so small that its effect on the stern was almost insensible, and the united resistance was due to the generation of a separate wave system by each end of the ship. "As we gradually reduce the length of middle-body the stern is brought within reach of waves large enough to produce a sensible effect, and according as it is brought into conjunction with a crest or a hollow, the total wave-making resistance becomes alternately less or greater than that due to the sum of the actions of the two ends of the ship when acting independently; the wave-making resistance becoming least of all (except at the very highest speed) when the middle-body is reduced to nothing."

Mr. Froude points out that the length spacing of the wave system, and consequently the positions of the troughs and crests, depends on the speed, and therefore the position of the after-body, which is specially favorable at some given speed, may be specially unfavorable at a higher speed, and at a higher speed still may be favorable again.

The models experimented upon by Mr. Froude had fine entrances and runs, and this may account for the fact that the undulations in the curves of residuary resistances did not become important until comparatively high speeds were reached. Probably in ships having ends as full as an ordinary cargo-carrying merchant steamer, they arise at much lower speeds, and these phenomena must always exert a very considerable influence upon the behaviour of such vessels before and after being lengthened.

No part of the question of resistances possesses a more direct interest for shipowners than this, and we most heartily congratulate them and Mr. Froude upon the progress that has been made in our knowledge of the subject.

THE TRANSPORT OF "CLEOPATRA'S NEEDLE." *

By Mr. JOHN DIXON, C.E.

From "The Builder."

The obelisk known as "Cleopatra's Needle" is of great historical interest, because it had sculptured upon it the history of the man who quarried it from the old quarries of Syene (the modern Assouan), and the reason why he quarried it. There were few monuments extant, so far as we know, which could, like this, date back 3,400 years. In addition to its historical interest, however, the obelisk in question was of interest to engineers, in consideration of the means which were resorted to in order to quarry and transport in safety such huge monoliths. Cleopatra's Needle was not alone among obelisks, and possessed no peculiar features. In point of size, it stood only about eighth or ninth on the list of obelisks with which we were acquainted. The largest of which we knew was the Lateran Obelisk in Rome, which was brought by the Romans, with about twenty smaller ones, from Egypt, as the most curious objects they could lay hold of to decorate their imperial city. The Lateran Obelisk had a height of some-

* A paper read before the Civil and Mechanical Engineers' Society.

thing like ninety feet, and was ten feet six inches square at the base, whereas Cleopatra's Needle was only sixty-nine feet three inches high, with a base of seven feet square. Ten years ago, when the lecturer was in Egypt, his attention was especially directed to this obelisk, which he saw lying in the sand on the shore at Alexandria. He dug around it and under it for the purpose of examining it, and it appeared to be little the worse for wear except that two of its sides were somewhat weatherworn, and did not retain the polish which still existed on the other two sides. Nevertheless, the hieroglyphic inscriptions were quite distinct enough to be read by the learned in those matters, and it therefore retained its history as clearly as on the day when it was set up by Thothmes III., *circa* 1400 B.C. Egypt in those days was the leading country of the world, not only in arts and commerce, but in learning and science, and to her great university of Heliopolis came Strabo, Pliny, Herodotus, and others. Thothmes went to the old quarries of Syene for the material of his obelisks. From these quarries for generations before him the Egyptians had been accustomed to sculpture those great blocks of granite which even to this day were our wonder and admiration. The granite of Syene was micaceous and somewhat coarse in texture and pinkish in color, and in the quarries, to the present day, there existed an obelisk, half cut out, much larger than any other we knew of. That obelisk, if it had ever been completely quarried and set up, would have been ninety-six feet high and about eleven feet square at the base. The proportion of height of these obelisks to the square of base was generally about ten to one, or, in other words, the height was about ten times the square of the base. The hieroglyphics on "Cleopatra's Needle" were from two feet to three feet long, cut two inches deep and three inches or four inches broad, so that they could be read from a distance of fifty or sixty yards. The inscription, besides recording the virtues and power of Thothmes, recorded that the obelisk when set up was tipped with gold. This feature was of course gone, but there was a slight ledge round the point at the top which was no doubt made to receive a gold tip, or one of bronze gilt. On the top of the obelisk of Luxor at Paris, which the French brought from Egypt, there was a similar ledge or groove. The inscription on Cleopatra's Needle did not say in what city that obelisk was set up. Some accounts had it that it was first erected at Thebes, and was afterwards removed by Ramesis to Memphis. Be that as it might, it was ultimately removed to the ancient city of Heliopolis. The site of that city, once the Oxford and Cambridge of the world, and at that time, it may be presumed, covered with magnificent buildings, now presented one of the most astounding spectacles which could be witnessed. Nothing remained but a green plain, in the center of which was a solitary obelisk, opposite to which once stood the stone now known as "Cleopatra's Needle." The latter was subsequently removed to Alexandria. How it came to be thrown down in its present position nobody knew. It had been conjectured that an earthquake was the cause of its overthrow, but it was much more likely, Mr. Dixon thought, that it was thrown down to get at the bronze tortoises on which it was believed all these obelisks were placed. In 1798 the French conquered Egypt, and they laid their hands on everything that was valuable or invaluable in the country. They carried off an enormous quantity of Egyptian remains, and they proposed to carry off the Rosetta stone (now in the British Museum), Cleopatra's Needle, and other antiquities. Before they could do so, however, they were driven out of Egypt by the English, and at the conclusion of that brilliant campaign, in which Sir Ralph Abercrombie fell, a great effort was made to secure Cleopatra's Needle, which would when erected in London, form a fitting monument of one of the most brilliant campaigns in which English arms had ever been engaged. The army subscribed four or five days' pay, and, assisted by the navy, took steps to remove the obelisk. They had hardly commenced, however, ere the red-tape and pipeclay of those days sent forth an order to desist in the attempt, as such work would be destructive of discipline and of the accoutrements of the men. So the obelisk remained where it was. When Mehemet Ali assumed the reins

of power, he, wishing to please George III, presented the Rosetta stone and many of the principal objects in the Egyptian Court of the British Museum, together with Cleopatra's Needle, to the English nation. The British Government, however, had always declined meddling with this obelisk, although repeatedly urged to bring it over. The expense involved was made the great obstacle, although even so utilitarian a man as Joseph Hume proposed to spend the national money in bringing over the obelisk, and contended that the money would be well spent for such an object. The cost of bringing over the obelisk has been estimated in years gone by at £100,000, but Mr. Dixon said he was confident that the cost would not exceed one-eighth or one-tenth of that sum. Eventually the British Government had renounced the gift. Recently, as was well known, Dr. Erasmus Wilson, F.R.S., had munificently come forward and had offered to find the money if Mr. Dixon would undertake the engineering details involved in the transport of the obelisk to England. That offer had been accepted. The Kedhive, on being spoken to on the subject by Mr. Fowler, his Highness's chief engineer, and who was now in Egypt, said he should be pleased to see the obelisk removed to England if the Government would accept it on behalf of the nation. This the Government had consented to do, and it had again been presented to England through our Consul-General in Egypt. Nothing now remained, therefore, but to remove the obelisk to England. The stone was at present lying embedded in the sand on the shore of Canopus Bay, to the east of Alexandria. It was parallel to, and not far from the water-line, which washed the foot of a quay-wall backed up by the sand in which the obelisk was buried. The water was very shallow for a considerable distance out, and its bed was of rock covered with a fine sand, so that to dredge or excavate a channel for a vessel to come alongside the quay-wall and take the obelisk on board would involve an outlay of £50,000 or more. It would also be impossible to make a mole on which to convey the obelisk out to a vessel lying in deep water, simply because there was no material available, and even were there material, the length of mole required would necessitate an outlay of at least £30,000. Neither of these plans was available. What was proposed to be done was simply to excavate about the obelisk, and to build around it a huge iron cylinder or boiler, so to speak, ninety-five feet long and fifteen feet in diameter. In the first instance there would be constructed around the obelisk, ten feet apart, about seven diaphragms, discs, or collars, so to call them, circular on plan, and with square aperture in the center through which the obelisk passed, and would be held in position by wedges and other appliances so disposed and arranged as to prevent any excess of strain being brought to bear on the monolith under any circumstances, whether the cylinder containing it were held up in the center on the crest of a wave or at each end by two waves without any support in the center. To these diaphragms or collars would be built longitudinal girders and framing, which would in their turn carry concentric ribs of iron to receive the external skin of plate iron.

The obelisk will be disposed as nearly as possible in the center of this cylinder, which will have both its ends pinched up, so to speak, in wedge-form, to serve as bow and stern. On the side of the cylinder which is intended to remain uppermost manholes will be left for taking in ballast. The weight of the obelisk is about 183 tons. The cylinder, when completed, will be rolled out in the shallow water until it floats, being previously covered with a stout jacketing of timberwork, in order to prevent the possibility of its being damaged by any sharp pieces of rock which it may roll over. When the cylinder is in deep water it will be towed into the Khedive's dry dock at Alexandria, where the wooden jacketing will be stripped off; one of the wedge-shaped ends will be provided with a rudder, and the cylinder will be fitted with two bilge-keels and a small platform or deck, thus becoming a cylinder ship. It will be named the *Cleopatra*. Ballast will be taken on board to give the whole greater stability, which would have been secured, if the obelisk could have been taken on board an ordinary vessel, by placing the load nearer to the bottom than in the present case, where, in order to facilitate the rolling of the cylinder,

the obelisk is to be placed in the center. All being ready and the weather favorable, the cylinder and its contents would be towed to England. Mr. Dixon went into details to show the stability of the vessel, as to which every care had been taken, seeing that not only was the safety of the obelisk involved, but the credit of English engineering was at stake. The plans of the vessel had been approved by Mr. Froude and other authorities on the subject of naval architecture. Having reached the Thames it would be towed to that part of the Embankment wall nearest to the site which might be selected, and at high tide would be floated on to a staging or gridiron of timber constructed to receive it. When the tide receded the cylinder would be left high and dry, and would then be raised by hydraulic power until the level of the embankment was reached. The keels, deck, and other excrescences having been removed, and the ballast taken out, the cylinder would be rolled across the Embankment to the site the obelisk was intended to occupy where the cylinder would be broken up and removed to the scrap-heap. Then came the question of raising the obelisk into an upright position on its pedestal. The Romans, and, later, the French, had pulled their obelisks up by ropes, but that plan necessitated a large number of capstans and other appliances. Mr. Dixon proposed to raise Cleopatra's needle in the following manner: Having brought the center of the obelisk over the exact site it was to stand upon, he should put round the central four feet or five feet an iron jacket, or, more properly speaking, a pair of iron stays capable of being so tightly laced or screwed up as to prevent the stone from slipping. On the centers of two of its sides this jacket would have projecting trunnions capable of bearing the whole weight of the obelisk.

The obelisk would then be gradually raised by hydraulic power, applied at each end alternately, and as it was raised a scaffold or staging of bulks of timber would be gradually built under it, the obelisks still lying in a horizontal position. When the timber staging had reached the required height, two iron girders would be placed parallel with the obelisk. Upon these girders would rest the trunnions before referred to, and the girders, resting upon the end portions of the timber-staging, would allow of the central portion of the staging being removed, so that the obelisk would swing nicely balanced on the trunnions, the bottom of the obelisk just clearing the stone on which it was to rest, and from that position could be easily lowered into its place by the lowering of the girders to a slight extent. One advantage of this plan was that on a bright moonlight night the stone could be experimentally lowered on to its pedestal to ascertain whether everything was right, and could then be pulled up again ready for the public ceremony. The lecture, which was illustrated by large plans on the walls, and by a model of the proposed cylinder-ship, attracted an unusually large audience, and an interesting discussion ensued.

PROFESSOR TAIT ON FORCE.

By ROBERT D. NAPIER.

From "Engineering."

The importance of having clear conceptions of the meaning of the terms used in reference to questions in dynamics, and Professor Tait's lecture on "Force" at the last meeting of the British Association, having deplorably intensified some of the most pernicious errors of ordinary writers on that science, which I undertake to prove he has done, is my excuse for asking you to insert this letter.

We are told in most treatises on dynamics that the same symbol represents both the acceleration produced by gravity and the force which produces the acceleration. I find in the preface to Young's Mathematics (1862) the following sentence, quoted from De Morgan,

which should be printed in red letters in every treatise on dynamics: "Again, accelerating force which any one would suppose is the force which produces the acceleration, is no such thing; it is the effect produced, the very acceleration itself. I dwell upon this from a recollection of the confusion it created in my own mind when a student of the subject." This will show that I am not singular in protesting against this error, which I had all along supposed to arise from carelessness, and from a notion that it was somehow shorter and easier than to state the truth, that an effect can never be the cause of the effect produced. This mischief done by this gross carelessness, as I previously considered it to be, has been incalculable and lamentable, and the slovenly way of putting the case is inexcusable. However, we are now told by Professor Tait in his lecture, of which I had not till lately seen a full report, that in order to properly understand the subject, it is essential that we should clearly understand that "force *is* the rate of change of momentum," so that if a body is screwed up in a vice there is no force acting on either side of it; and yet we are told that we are to use the word force in exactly the same sense as Newton used it, and that it is obviously to be applied to any "strain, stress, pull, push," &c. We are also told that "any change in the rate of motion of a body is to be attributed to force," and that "momentum is mass × velocity." Now substituting in this sentence velocity for rate of motion, and the definitions of force and momentum for the words, we get the no doubt valuable, if obscure, information that *any change in the velocity of a body is caused by the product of the rate of change of its velocity × its mass.*

We are further informed that "force is a mere name," but that "the product of force into the displacement of its point of application has an objective existence." It is rather curious how passing a name over a distance has such surprisingly creative effects. Probably passing this created objective existence over a surface might create matter, and passing this matter through a volume might produce life. Huxley should look into this.

To illustrate his subject Professor Tait tells us that "a raised mass, in virtue of its elevation, possesses an amount of energy precisely equal to the work spent in raising it. This dormant or passive form is called potential energy." Is not the dormant energy somewhat contradictory in terms? Potential energy I can understand, but is it "stored up in the raised mass," as we are told in this lecture? An answer to this question may be found in another illustration given. "When we draw a bow we do work," and "the drawn bow has in it potential energy" . . . . "which can be expended on the arrow." If the work done in raising the weight is stored up in the weight, the work done in drawing a bow must surely be stored up in the arrow, which occupies an exactly similar position to that of the raised weight. If the work is stored up in the weight, how does it show itself? It must either be in the velocity of the body, or in its heat, or in its molecular condition; but none of these are altered any more than they are in the arrow when the bow is drawn back. It is the force of gravity that has been overcome through a definite distance—the body is totally unaffected; two forces were acting in opposite direction on it, but the body was as passive as the arrow. Force can no more exist without a resisting force than a rope can be stretched out horizontally unsupported with one end loose. After what has been said the following sentence hardly requires any comment. "We have just seen that when work is spent against molecular forces, as in drawing a bow or winding up a spring, it is stored up as potential energy," and "it is stored up in a similar form when done against gravity as in raising a weight." When the bow is drawn out or the spring wound up both have their condition altered, but in raising a weight its condition is not altered but only its position. When a man climbs a ladder, he finds he has lost energy and not gained it, and if a weight could do the same the result would be the same.

Its potential energy in reference to some position assumed to the stationary will vary as the square of its velocity in relation to that supposed fixed point, but the whole of the dynamical energy inherent in the body, is its inertia, or power of resistance to alteration of velocity.

TERRESTRIAL MAGNETISM AND THE MAGNETISM OF IRON VESSELS.

By FAIRMAN ROGERS.

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II.

MAGNETISM OF IRON VESSELS.

20. It has been supposed that the magnetic condition of certain rocks, well known as affecting the action of the needle when the compass is used on shore, would interfere with its action on the water, but although the compasses of a ship in a dock, surrounded by large masses of iron or moored in very close proximity to magnetic rocks, will be affected there is no reason to suppose that any rocks act upon the compass at the distance within which vessels ordinarily approach the shore.

A compass placed upon a ship built wholly of wood, would be subject only to the variations which we have considered, and they are now determined with sufficient accuracy to prevent any danger occurring from their interference; but in a vessel, in the construction of which iron is used to a greater or less degree, or in a steam vessel, there are disturbances of magnetic condition which are of the utmost importance.

21. In the first place, soft iron without any magnetism of its own affects the compass (19), since under certain conditions it becomes temporarily a magnet; while iron which has acquired any degree of magnetism either by position or by manipulation acts upon the needle in a still more powerful manner. If we balance a long needle in such a way that it may move freely in both a horizontal and a vertical plane, and then magnetize it, it will turn into the vertical plane of the magnetic meridian, and will dip in that plane according to the magnetic latitude of the place of experiment, (16).

If we suspend a bar of soft iron in the same position it will immediately acquire magnetic force, which it will lose again in proportion as it is moved out of this position, (19). In consequence of this, iron in a vertical position frequently affects the compass to a great extent. In the case of U. S. Sloop of War, Ticonderoga, examined in 1863, the standing mizen rigging which was of iron wire, acted so powerfully and in such a varying manner upon the steering compass, which was in a line joining the mizen chains, that it was found necessary to remove it and substitute hemp rigging before the compass could be corrected in any satisfactory manner.

In fact all the deck beams and pieces of iron in a vessel become magnetic to a greater or less extent, and as they change a portion of their magnetism with changes of position, they act differently upon the compass as the ship heads in different directions; and as they slowly lose or gain, relatively or absolutely, their action changes also as time elapses.

22. As the effect on the compass in such cases is the resultant of the action of a large number of pieces of metal in different positions, the determination of this effect must always be a matter of experiment with each particular vessel and for each position on it; but the consideration of the action of certain masses in a few positions will lead to results that may enable the officer to select the position in the ship which will be most free from disturbances, and frequently to make such dispositions as may neutralize the peculiarily bad effects of certain arrangements.

It will also enable us to correct the compass empirically, by introducing disturbing causes which will nearly balance those arising from the material of the vessel.

23. We must be careful to distinguish clearly between variation and deviation.

The *variation* is the amount by which the compass points E or W of the true North, by reason of the want of coincidence of the magnetic pole with the pole of the earth (6) the *deviation* is the amount by which the compass deviates from the *magnetic* North and depends upon the ship's action and position.

24. In one sense we may consider an iron vessel as a magnet which acts upon

the needle as the earth does, and the needle will in that case be acted upon by the combined forces of the earth and the ship, and will assume a position depending upon the mechanical resultant of those forces.

Thus if the ship is so magnetized that the north end of the needle is attracted toward the bow, *and* the ship is heading to the magnetic North*, the action of the ship and the earth being in the same direction, the needle will not be disturbed. If, now, the ship alters her course, so as to head East, the needle will be drawn by the earth's force toward the North, and, by the ship's force to the East, and must, therefore, assume a position between the North and East, depending upon the relative amount of the two forces.

The amount by which the needle is drawn from the magnetic North is called the deviation, and in the case which we have supposed, this deviation will change with azimuth of the ship's head, being zero when the head is to the North, and attaining a maximum when the head is toward the East or West.

It is this deviation that we wish to study so as to be able to predict it, to allow for it, or to correct the compasses so as to eliminate it. The action of the ship upon the compass is not, however, so simple as above supposed.

The force of the ship is never exactly to the head, but in a direction making some angle with the axial line of the ship, and therefore, for convenience, we divide this force, as we do that of the earth, into two components at right angles to each other, viz., the ship's *force to head*, and the ship's *force to starboard*, as *ab* and *ac*, Fig. 1, *ad* the resultant representing in direction and amount the ship's force.

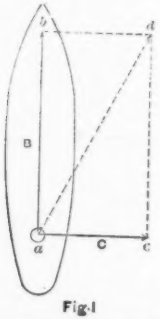


Fig. 1

We may also separate the *vertical* component of the ship's force but we shall not consider that at present.

26. To determine the amount and direction of the ship's horizontal force, any compass may be used, the needle of which traverses freely, but the more delicate the instrument the more satisfactory will be the results.

Place the compass in a position on shore free from any local attraction, and by means of a small magnet or a piece of iron deflect the needle 90° from the north point, permit it to vibrate and count the number of vibrations in a minute, repeating the observation several times to ensure accuracy. If the number of vibrations of the different sets do not agree within a quarter of a vibration either the instrument or the observer is at fault.

This number of vibrations in a minute gives a measure of the earth's horizontal force at the place. Then take the *same* compass on board the ship and count the number of vibrations in a minute made there. The ship's force with her head in the particular position at the time of the observation and at the place occupied by the compass will be to the earth's force as the square of the number of vibrations on board is to the square of the number on shore. A single observation on board will give a result, but it is much better to make a number with the ship heading in different directions, for the magnetism of the soft iron changes, as we have seen with changes of direction.

To show how the amount and direction of the ship's force may be plotted and determined, we give an example at length, when the ship's force is greater than that of the earth, and the condition therefore somewhat exaggerated.

In this case the ship is represented by a rectangle of cast iron, which has become permanently magnetic, the compass being placed in the middle of it and one end of the rectangle marked as the bow.

No. of vibrations on shore, that is not surrounded by the iron representing the ship, 9.5 in a minute. Bow to the North, No. of vibrations, 20.5; Needle pointing

* It will be understood that in this treatise, North always means Magnetic North, unless otherwise stated.

6° W of N. Bow to the East, No. of vibrations, 18.75; Needle pt. 72° E of N. Bow to the South, No. of vibrations, 16.5; Needle pt. 7° E of S. Bow to the West, No. of vibrations, 18.40; Needle pt. 84° W of N. Now, as in these positions on board ship the needle is under the influence of the ship and the earth both, these directions and vibrations are due to the combined influence of earth and ship, and represent in direction and amount the resultant of the forces of earth and ship, and the forces are to each other as the squares of the number of vibrations, according to the law of the pendulum.

We have, therefore,

On shore,	$9.5^2 = 90.25$		
Head N,	$20.5^2 = 420.25$	Amount of resultant force	6° W of N
Head E,	$18.75^2 = 351.00$		72° E of N
Head S,	$16.5^2 = 272.25$		7° E of S
Head W,	$18.40^2 = 338.56$		84° W of N
		Direction of resultant force.	

We may now plot this so as to obtain the amount and direction (with reference to the ship) of the ship's force. In Fig. 2 let C be the position of the compass, the north being to the top of the page, CN′=90.25 representing in direction and amount the force of the earth, and C N =420.25 N 6° W, representing in direction and amount the resultant of earth and ship, the force exerted by the ship will, therefore, be represented by CO=330, and 8° to Port of center line of ship.

So with head E, CE=351, N 72° E represents resultant of earth and ship, and CP the force of ship 335, 2° to Port of center line.

Again, with head S, C.S=272.5 S 7° E. and CO=368, 5° to Port of center line.

With head W, CW=338.56 N 84° W and CR=440, 9° to Port of center line.

Therefore, ship's force is $\frac{330+335+340}{3}$ =335 in amount as compared with earth's force, 90.25 or 3.71 earth's force being 1.00 and $\frac{8+9+2+5}{4}$=6° to Port of center line in direction, the line passing through the position of the compass.

In taking the average of the amounts, the value to the S 368 is omitted, because the action of the earth and that of the ship being directly opposed, the needle is sluggish and its number of vibrations cannot be depended upon. This is an extreme case, since usually the ship's force is less than that of the earth, but it is selected because it shows the method better in plotting.

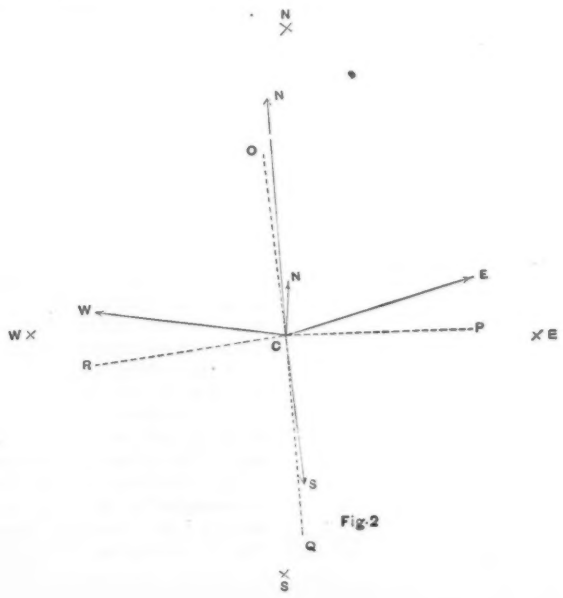


Fig. 2

Of course, these values can be calculated by the ordinary means if greater accuracy is desired. It is obvious, also, that if only one or two observations can be made they are better with the head E or W than N or S.

It must be remembered that the direction and amount of the ship's force varies with her position within certain limits, and that one line can, therefore, only represent the mean force and direction.

Part of her force comes from her soft iron, and in this paragraph (26) we have considered that only her permanent magnetism is affecting the needle.

In Fig. 2, the line CE is the resultant (or diagonal of the parallelogram) of CN′ and CP; knowing CN′ and CE we construct the parallelogram CN′ EP and thus find CP which is required, and the same with CQ CR and CO.

Although the ship acts upon the compass as a whole, like a magnet; still it is not like a simple magnet but one which changes its magnetism in different positions, and in fact its action is made up of the separate action of different parts.

27. The investigations of Poisson and Airy, show that part of the action of the ship is due to the permanent magnetism of the hard iron, and a part to the induced magnetism of the soft iron, (18) and the action upon the needle or the deviation can be expressed by a mathematical formula. The formula given by Poisson is more complete in some respects than that given by Airy, but the latter is simpler and is as follows:

$$S = A + B \sin. z' + C \cos. z' + D \sin. 2z' + E \cos. 2z'$$

in which S is the deviation to be determined.

Z' is the azimuth of the ship's head by the disturbed compass.

And A, B, C, D, E are coefficients whose values are determined by experiment.

Table I gives an analysis of these coefficients and should be carefully studied.

28. A is constant for the same ship and compass, and is real or apparent as shown in the table.

It is generally very small except in the case where the compass is not in the middle line of the ship. All these coefficients are positive (+) when they act towards the head or the starboard side, negative (−) when they act to the stern or to port.

29. B and C together represent a force which acts as represented in Fig. 1, and which has its zero when the ship's head is in a certain direction, (usually towards the North), and its maximum when the head is at right angles to that direction, from which peculiarity it is called the semicircular part of the deviation.

It is caused: I, by the permanent magnetism of the hard iron, and as that is constant it varies inversely as the earth's horizontal force; II, by the induced vertical part of the earth's force on soft iron, and therefore changes with magnetic latitude as the dip changes.

As this force rarely acts in the direction of the ship's length we divide it into two, B, acting fore and aft, and C acting athwart ships. (5 of the table).

30. D and E represent a force which is due to the horizontal induction of soft iron, and which is called the quadrantal part of the deviation for the following reason.

If a bar of soft iron is held in the direction of the dip it becomes a magnet, the upper end of which, in North latitude, attracts the N end of the magnet, (21) if it is moved into a horizontal position, but still pointing North and South it is still magnetic, under the influence of the horizontal component of the earth's force though in a smaller degree. If it is now moved horizontally into an East and West position, its magnetism still further diminishes and when it reaches the East and West position its magnetism disappears altogether.

If, therefore, a compass is acted upon by this bar, when the bar is North or South of the needle, it attracts it strongly, but as the needle is already pointing North, its directive force (7) is increased, but its direction is not disturbed; if the bar is carried around radially, say to the East, the needle is drawn toward it and so has an Easterly deviation.

But the bar as it approaches the E and W position, loses its magnetism; and when E and W has none, and therefore does not attract the needle, and produces no deviation; consequently, the deviation will be zero when the bar is N and S and N of the compass, zero when the bar is

Deviation of the Compass in Iron Ships.

$$\delta = \mathbf{A} + \mathbf{B}\ sin.\ Z' + \mathbf{C}\ cos.\ Z' + \mathbf{D}\ sin.\ 2Z' + \mathbf{E}\ cos.\ 2Z'.$$

δ = *deviation.* Z' = *azimuth of ship's head by disturbed compass.*

Constant: A

- **Real** — *from induction of* horizontal *force on* soft *iron unsymmetrically distributed.*
  - \+ *When Easterly deviation is in excess.*
  - — *When Westerly deviation is in excess.*
- **Apparent:** *from* index *or obs. error.*

A *is common in character and value in all classes of vessels. It may occur in heeling, from the propeller shaft being thrown to one side, but it is very small.*

May also occur from horizonal spindle of steering apparatus.

Semi-circular *or* **Polar magnet.** *from*

1. *Permanent magnetism of* hard *iron, varying inversely as the* Horizontal force *at the place.*

2. *That induced by the* vertical *part of the earth's force on* soft *iron, varying as the* tangent *of the dip.*

B *is that part of this combined attraction acting in a* fore and aft *direction (the nat. sin. of B is* ship's force to head).
- \+ *if* forward *of the compass.*
- — *if* aft *of the compass.*

C *is that part of this combined attraction acting athwart ship (the nat. sin. of C is* ship's force to starboard).
- \+ *if to* starboard.
- — *if to* port.

The resultant of B *and* C *is the* ship's force, *and is in its direction*

$$\left[\tan.^{-1}\frac{C}{B}\right]$$

and amount

$$\left[\sqrt{B^2+C^2}\right]$$

equivalent of the "true starboard angle."

In the wood-built *vessel (in Great Britain)* B *is* + *when the engines are* forward *of the compass, and* C *is always small in value. In the* iron built *vessel* B *is irrespective of machinery*
- \+ *When the ship was built* head South.
- — *When the ship was built* head North.

and nearly vanishes when head was E. *or* W.

and C *may be large and either* + *or* —.

1. *In an* iron *vessel built in England* head South, *the* N. *end of the needle is drawn to the* bow *or* B *is* +. *The machinery has the* same *effect and must be* added.
2. *Ditto, head to the* North, *the* N. *end is drawn to the* stern, *or* B *is* —. *The machinery has the* contrary *effect, and must be* subtracted.

The semi-circular deviation is sensibly diminished by raising the compass to a short distance above the deck beams.

As it depends upon the dip it is **0** *at the magnetic* equator, *and a* maximum *at the magnetic* poles.

Quadrantal *sometimes called* permanent *coefficients, because unchanging in all magnetic latitudes.* *due to* horizontal *induction of* soft *iron in ships.*

D *has four maxima at* intercardinal *points.* *Symmetrically distributed.*
- \+ *When from masses acting in* fore and aft *direction.*
- — *When from masses acting in* transverse *direction.*

E *has four maxima at* cardinal *points, and is generally so small that it may be neglected.* *Unsymmetrically distributed.*

D *ranges in* iron *vessels from* + 1½° *to* 7° *(even* 11°*), and in* wood, screw *vessels, seldom exceeds* + 1° *(sometimes* —*), and in* wood, side-wheel *vessels* + 1½°.

E *is common in character and value in all classes of vessels.*

D *has the following characteristics:* 1. *Invariably* +. 2. *Its amount does not depend upon the size of the vessel or* direction *when building or on* iron beams. 3. *It gradually* decreases *with time.* 4. *It is* unchanging *in all magnetic latitudes.* 5. *Its value is from* 2° *to* 4°.

The quadrantal deviation is diminished by raising the compass to the mast head, but not by changing within two to six feet from the deck.

E and W and E of the compass and will have a maximum somewhere between N and E.

It will be the same in each quadrant except that in the second quadrant E to S the S end of the needle will be attracted by the N end of the bar, thus giving a W deviation. E in the S to W quadrant and W again in the W to N quadrant. See Fig. 3.

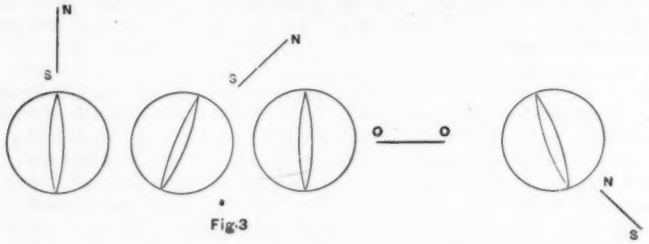


Fig.3

The top of the page represents the North.

This quadrantal deviation is divided into two parts, D, which is the more important, and which has its maxima at or near the intercardinal points NE, SE, SW, NW and E which has its maxima at the cardinal points N, S, E, W, and which is generally so small that it may be neglected. It arises from soft iron unsymmetrically distributed.

31. Of the five coefficients, A and E are small, and B, C, and D are the important ones to consider, and those we shall therefore discuss at greater length.

A portion of the action which introduces the coefficients B and C is, as already noticed, due to the permanent magnetism of the hard iron composing the ship.

A piece of hard iron or steel may be made a permanent magnet by touch with another magnet, by subjection to the action of a current of artificial galvanism or by being placed in such a position that the currents of the earth will magnetize it.

The last is the only method by which a vessel will be magnetized, and as the currents of the earth have a definite relation in their direction to the earth itself, it is evident that the magnetism of an iron vessel will vary with the position which the ship has during its building, or in the case of an iron clad, during its plating. Late investigations* have indicated that the North end of the needle is attracted toward that part of the vessel which was nearly South in building.

That this magnetism is more constant in vessels built North and South than in those built East and West.

That the diminution of the directive force is greater if the ship has been built E and W, than if built N and S.

That the deviations in a ship built E and W are less symmetrical, regular and permanent than those on a ship built N and S. From which it is evident that it is better to build ships N and S, than E and W. When the compass is to be placed in the after part of the ship, as is the case with most of our war vessels, it is best to build the vessel head to the South, because if the ship is built head North the upper part of the stern, and the lower part of the bow, will be strongly magnetized, and the lower part of the stern, and the upper part of the bow weakly, so that a compass placed near the stern will have a large semicircular deviation.

If the compass is amid-ships the direction in building is not important, and if the compass is forward as in some merchant propellers, the head should be North in building.

32. The amount of magnetism upon which B and C depend is affected by the treatment which the vessel receives during building, and after being launched and in the early part of her service.

If in a vessel to be plated, built with the head North, a certain amount of magnetism is introduced by her position, another amount having an opposite effect upon B and C may be introduced by plating the vessel with her head South, and in this way the semicircular deviation of an after compass (31) may be much diminished, annulled or reversed

* Evans and Smith, Phil., Trans. Pt. I, 1865.

by this operation, a fact of great importance, since although the position for building cannot often be chosen—such a position for plating as will modify this element of the deviation, can almost always be selected by choosing one or the other side, or the end of a wharf, and mooring the vessel bow in or bow out.

No case has been reported, within the knowledge of the writer, in which the complete experiment has been made, but the observations on the Warrior, Black Prince, Defence, Resistance and Valiant (Evans and Smith, Phil., Trans. Pt. I 1865) where the directions before and after plating were different, have given just the results which might have been anticipated.

Again the permanent or subpermanent magnetism is affected by the jars or blows to which the iron may be subjected; thus if (19) a bar of iron held in the direction of the dip, be struck with a hammer it will become more highly magnetized than if it is held quietly; and shaking or pounding when in an E and W position will cause it to give up some of its charge.

For the first few months after launching, during which time the vessel is exposed to the hammering of the workmen in completing the work, to the vibrations of the machinery, and to the shocks of the waves, the values of B and C change, generally diminishing until they attain a permanent value in about one year.

Swinging at anchor through all azimuths, or merely lying quietly in a direction differing considerably from that of building, will also cause some changes.

It is evident that this magnetism is not exactly permanent as that of a steel magnet is, and it is therefore sometimes called subpermanent.

33. It follows from these considerations that we may by judicious management, so modify the action of the hard iron, as in many cases to reduce the value of B and C to within small limits as far as they depend upon the permanent magnetism, and that this magnetism becomes settled or *shaken down* at the expiration of twelve months.

34. Now the intensity of the earth's attraction of the needle to the North depends upon the magnetic latitude (16), and the ship's action being permanent in amount, the total directive force upon the needle will vary with the latitude, the effect of the ships becoming apparently greater as that of the earth becomes less, and B and C will therefore increase inversely as the horizontal force at the place, and will increase as the ship goes from the equator towards the poles.

35. There is another part of B and C (No 2 Table 1), that induced by the vertical part of the earth's force on soft iron of the vessel, which varies as the tangent of the dip; that being the representative of the vertical part.

This can be separated from the first by treating observations made in different latitudes, or when the vessel is on an even keel and when she is heeled as in the last case, the transverse iron, such as beams, posts, and the ship's sides, change their induced or temporary magnetism as they change their inclination.

The difficulties of heeling a vessel are however very considerable, and up to this time observations have not been made with sufficient differences of latitude to give very good results, so that 1 and 2 are not well separated.

The change of induced magnetism in the soft iron of the vessel is of great practical importance, however, in another respect, for when an iron sailing vessel or a steamer under sail, is careened by the wind, the deviation is sometimes much affected temporarily, and an error sometimes quite large, introduced, called the heeling error.

36. So far we have only considered the deviations while the ship is on an even keel, but they may vary when she heels to port or starboard, and as in some localities, a vessel may sail for days with the wind on the beam, this unexpected addition to the deviation may carry her out of the way.

The heeling error is produced by the action of the permanent magnetism, and by that of the induced magnetism of the transverse soft iron.

The direction of the ship's force is not necessarily horizontal, (25), in fact it is nearly always inclined more or less in the vertical plane. If it is of such a character as to draw the N end of the needle downwards, in the position of compass, it is said to act downwards, and it is evident that in this case, when the ship heels, the N end of the needle will be drawn to the weather side.

Again, the transverse iron such as deck beams, become magnetic as they incline, and their upper or weather ends attract the north point of the needle, thus further increasing the heeling error. If, however, the ship's force at the compass acts upwards, the needle is attracted by that to the lee side, and by the induction of the deck beams to the weather side, and thus the heeling error is reduced or practically destroyed. This is one strong argument in favor of building vessels head South in North latitudes, since such ships usually have an upward force in the after part where the steering compass is placed.

The effect of the induction will be at the magnetic equator, and will be reversed, as the upper end acquires magnetism south of the equator. Therefore, as far as this error is concerned, ships built to run in South magnetic latitudes, should be built head North.

This heeling error may be compensated on the same principle as the other deviation (46), by placing a vertical magnet in such a position, vertically under the compass, that it will produce no deviation when the ship is on an even keel and just enough of an opposite kind as the ship heels to counteract that produced by the ship itself. This will not be entirely satisfactory, since the error from induction in the soft iron will change with the latitude but it will reduce the error considerably.

Owing to the difficulty of heeling a vessel, there are not so many observations on this point as is desirable, and they should therefore be made whenever opportunity occurs. In all cases the angle of heel should be carefully determined and recorded with the results of the observations. In some vessels the error is very large, as in the City of Baltimore, 2° of deviation for each degree of heel. It is evident that as the heeling error due to the permanent magnetism is constant and that from induced magnetism varies with the magnetic latitude, observations of heeling error in the same ship and compass in different latitudes will enable us to determine the relative effects of the two, and such observations are very desirable, (35).

Vertical iron also produces a heeling error and in English iron merchant-men, where the steering compass is usually well aft., it has been found necessary to put up a vertical iron post forward of, and below the compass to compensate for the effect of the stern post. Tunnels, bulkheads, etc., frequently cause a heeling error.

The error is greatest in N and S courses, and least in E and W.

In compasses compensated by Airy's method with the magnets below the compass, the heeling error is much reduced.

Elaborate records of experiments in heeling vessels will be found in Appendix to Third report of Liverpool Compass Committee, 1862.

37. In considering the formula given in Section 27, it is evident that if we can get a sufficient number of equations with different values of δ and Z' depending upon each other, we may determine the value of A, B, C, D and E.

This can be done in the following way: The ship being at anchor in a place where there is no current or moored in a basin where she is not in the immediate neighborhood of large masses of iron or magnetic rocks, some distant object or objects must be selected, the correct bearing of which from the ship must be determined from the chart, or by careful observations *on shore* with an azimuth compass. The object must be distant, so that its bearing will not be sensibly changed by a small change in position of the vessel. (See Sec. 26 for other methods of determining deviation.)

The standard compass must then be set up on board at a point in the *midship* line of the vessel which has been selected upon general considerations hereafter to be noticed (49), or after several observations of the kind now to be described.

In most vessels a position between the main and mizen mast will be found to be the best.

The vessel's head being brought to any cardinal point, say to the North, by the standard compass, the bearing of the distant object by this compass must be read off and entered opposite the heading of the vessel as in column 2 of the table.

The difference between the true magnetic bearing of the distant object and its bearing by the compass will be the deviation, (δ) for that azimuth of the ship's head (Z') and must be entered in

3rd column of the table on the first line. The ship's head must then be swung towards the East, either by warps or with the assistance of a steam tug or boats until her head is N *b* E by the standard compass, the bearing of the distant point is again noted, entered opposite N *b* E, and the deviation on that course deduced. The same observations must be made all around the compass, except that in entering them, it is convenient, as will be seen, to make a fourth column, and begin it with the readings at South as on page 554.

In practice it will not generally be found necessary to *stop* the ship at each of these points, as the observation can be made at the moment her head is in the right direction, provided she is not swung too fast.

It is also quite possible to make the observations in a tide-way, which is not too strong, by taking advantage of the ship's swinging with the tide, although difficulty will sometimes be experienced in making her go all around at the next turn, instead of back through the side of the circle in which the first half of the observations were made.

For instance, if with the first tide the ship swings from N to South by the East, in many tide-ways she will, without some extraneous aid applied at the right time, be apt to swing back to N by the East, and not by the West.

A method (Napier's diagram) is given, (42) by which observations made on any azimuths, not necessarily N, N *b* E, &c. can be used.

This table of deviations may be used in practice to steer by, since it shows the error of the compass on each course, thus in the case of the Calypso when the ship heads N by standard compass the needle points 6° 14′ to the W of Mag. N and the ship would be sailing more than half a point to the W of N so that a corresponding correction must be applied to enable a true N course to be steered.

38. To proceed to the computation of the coefficients A, B, C, D, and E.

It is shown in the English Admiralty Compass Manual (Evans and Smith) by a mathematical demonstration too long to be introduced here, that the values of these coefficients may be deduced from the thirty two equations, giving the value of S by performing the operations indicated in the form given on page 554.

Prepare a form according to this example, the permanent part of which is indicated by the Roman letters and figures, and the quantities to be entered by the Italics.

Enter in the second and fourth columns the deviations as observed on each point, giving Westerly deviations this sign— and Easterly ones the sign +.

Form column V by entering the algebraic sums of figures in columns II and IV, having reference to the signs, and dividing by two; and form column VI by adding figures in cols. II and IV the signs of quantities in col. IV being changed, and dividing by two.

It will be seen upon reflection, and especially if the figures in these columns are plotted (see page 555) so as to aid the mind in comprehending these, that the figures in the sixth column are the means of the deviations in the semicircles from N to S, by the East, and from N to S, by the West, and represent therefore the mean semicircular deviations, and that those in column five represent the excess of the greater quantities over the mean, and represent therefore the quantities which affect the deviations, apart from the semicircular deviations, namely, the quadrantal deviations. On page 555 these are plotted, lines II and IV representing columns II and IV, and line VI the mean semicircular deviation.

Line V crosses the center line twice between N and S, and represents the quadrantal part of the deviation, line VI crosses once and represents the semicircular part.

If there were no quadrantal deviation lines II and IV would be nearly regular and symmetrical curves, equal on both sides of the axis, but the effect of the quadrantal deviation is to add to the semicircular deviation on the side towards which it acts, and to diminish it on the other side. For example, the mean deviation, Fig. 4, at N (or S) is −6°57′ (long dotted line VI) but II is drawn towards the plus side, to a reading of −6°45′ and IV is pushed to the same side to a reading of +7°09′. The difference between these readings, +0°24′ represents twice the action of the quadrantal deviation and $\frac{+0°24'}{2} = +0°\,12'$ repre-

Record of Deviations and Computation of Coefficents.

U. S. Steamer "Calypso," Acting Master, F. D. Stuart, August 17, 1865.

B and C.

I. Ship's head by disturbed compass.	II. Observed deviation of compass.	III. Ship's head by disturbed compass.	IV. Observ'd deviation of compass	V. Half sums of quantities in cols. II, IV. Unchanging part of deviation.	VI. Half sums of quantities in II, IV, signs of IV chang'd Semicircular deviation.	Computation of B. VII. Multipliers.	VIII. Products of col. VI by multipliers.	Computaion of C. IX. Multipliers.	X. Products of col. VI by multipliers.
N	— 6° 45′	S	+ 7° 09′	+ 0° 12′	— 6° 57′	0	0° 00′	1	— 6° 57′
N by E	— 6 00	S by W	+ 8 00	+ 1 00	— 7 00	S_1	— 1 22	S_7	— 6 52
N N E	— 4 21	S S W	+ 9 13	+ 2 26	— 6 47	S_2	— 2 35	S_6	— 6 16
N E by N	— 3 10	S W by S	+ 8 33	+ 2 41	— 5 51	S_3	— 3 14	S_5	— 4 51
N E	— 0 55	S W	+ 7 36	+ 3 20	— 4 15	S_4	— 3 00	S_4	— 3 00
N E by E	+ 0 51	S W by W	+ 5 30	+ 3 10	— 2 20	S_5	— 1 56	S_3	— 1 18
E N E	+ 1 20	W S W	+ 2 47	+ 2 03	— 0 43	S_6	— 0 40	S_2	— 0 16
E by N	+ 1 30	W by S	— 0 41	+ 0 25	+ 1 05	S_7	+ 1 04	S_1	+ 0 13
E	+ 1 50	W	— 1 28	+ 0 11	+ 1 39	1	+ 1 39	0	0 00
E by S	+ 2 00	W by N	— 3 09	— 0 34	+ 2 34	S_7	+ 2 31	$-S_1$	— 0 30
E S E	+ 2 20	W N W	— 6 11	— 1 55	+ 4 15	S_6	+ 3 56	$-S_2$	— 1 38
S E by E	+ 2 50	N W by W	— 8 40	— 2 55	+ 5 45	S_5	+ 4 47	$-S_3$	— 3 12
S E	+ 3 55	N W	— 9 10	— 2 37	+ 6 32	S_4	+ 4 37	$-S_4$	— 4 37
S E by S	+ 4 00	N W by W	—10 00	— 3 00	+ 7 00	S_3	+ 3 53	$-S_5$	— 5 49
S S E	+ 4 50	N N W	— 9 06	— 2 08	+ 6 58	S_2	+ 2 40	$-S_6$	— 6 26
S by E	+ 5 22	N by W	— 8 05	— 1 21	+ 6 43	S_1	+ 1 19	$-S_7$	— 6 35
					Sum of + terms =		+ 26 26		+ 0 13
					Sum of — terms =		— 12 47		— 58 17
					divided by 8)		+ 13 49		8) — 58 04
					B =		1° 43′	C =	— 7° 16′

Easterly deviations +

Westerly " —

A, D and E.

XI. Upper half of col. V.	XII. Lower half of col. V.	XIII. Half sum of quantities in cols. XI, XII Const'nt part of deviation.	XIV. Half sum of quantities in cols. XI, XII with signs of XII changed Quadrantal deviation.	Computation of D. XV. Multipliers.	XVI. Products of col. XIV by multipliers.	Computation of E. XVII. Multipliers.	XVIII. Products of col. XIV by multipliers.
+ 0° 12′	+ 0° 11′	+ 0° 12′	0° 00′	0	0° 00′	1	0° 00′
+ 1 00	— 0 34	+ 0 13	+ 0 47	S_2	+ 0 18	S_6	+ 0 43
+ 2 26	— 1 55	+ 0 15	+ 2 10	S_4	+ 1 32	S_4	+ 1 32
+ 2 41	— 2 55	— 0 07	+ 2 48	S_6	+ 2 35	S_2	+ 1 04
+ 3 20	— 2 37	+ 0 21	+ 2 58	1	+ 2 58	0	0 00
+ 3 10	— 3 00	+ 0 05	+ 3 05	S_6	+ 2 51	$-S_2$	— 1 11
+ 2 03	— 2 08	— 0 02	+ 2 05	S_4	+ 1 28	$-S_4$	— 1 28
+ 0 25	— 1 21	— 0 28	+ 0 53	S_2	+ 0 20	$-S_6$	— 0 49
Sum of + terms =		+ 1 06	Sum of + terms =		+ 12 02		+ 3 19
" — " =		— 0 37	" — " =		— 0 00		— 3 28
divided by 8)		+ 0 29	divided by 4)		+ 12 02	4)	— 09
A =		+ 0° 03.′6	D =		+ 3° 00′	E =	— 0 02′

sents the quadrantal deviation at N (or S).

The reader can make this still clearer by plotting from a center line, any regular semicircular deviation, and any regular quadrantal deviation, and then plotting the resulting deviation course (Fig. 5), an operation the converse of the above.

In Fig. 4 the line VI is the mean of the two lines II and IV, IV having been turned over on to the same side of the axis as II, or having its *signs changed* (Col. VI).

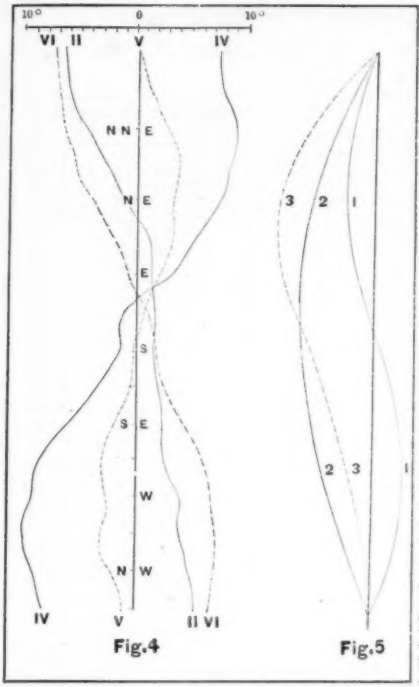


Fig.4 Fig.5

38. Having thus separated the semicircular and quadrantal deviations, we proceed to compute B by multiplying each number representing the deviation in Col. VI by the natural sine of the rumb of the course counting from the North, that is by O, S_1, S_2, &c., which are taken from the following table :

S_1 = nat. sin.	11°	15′	= .19509	1	point.
S_2 = " "	22	30	= .38268	2	points.
S_3 = " "	33	45	= .55557	3	"
S_4 = " "	45	00	= .70710	4	"
S_5 = " "	56	15	= .83147	5	"
S_6 = " "	67	30	= .92388	6	"
S_7 = " "	78	45	= .98078	7	"

In the English Admiralty Compass Manual, a table is given of the products of S_1, S_2, &c., with arcs up to 35°, which facilitates the operations very much.

These values are entered in column VIII.

The sums of the + terms, and of the − terms in this column are then taken and their difference with its proper sign + or −, divided by 8 gives the value of B in arc, as in the example B = +1°43′. We then form column X by multiplying the same quantities by the nat. sines of the rumbs, counting from E or W, to get C, which acts at right angles to B, the quantities S_1, S_2, &c., being entered in the inverse order.

The algebraic sum of the quantities in X divided by 8 gives the value of C, in example, C = −7° 15′.

Then to obtain the values of D and E, the quadrantal coefficients we take column V and entering the quantities in the upper half in column XI, and those in the lower half in column XII, since we wish to get the mean of the quadrantal deviation, we perform in columns XIII and XIV, the same operations as in columns V and VI, eliminating the constant or index error A, and getting quantities in column XIV to be multiplied by S_2, S_4, S_6, etc., computing D and E as shown in columns XVI and XVIII. In the example A = +0° 03′6 D = +3°00′E = −0°02′.

In case the observations are made on 16, instead of 32 points, that is at N., N.N.E., N.E. etc., the columns will be only half as long, and the divisions for B. C. and A will be 4, and for D and E, 2, instead of 8 and 4, but the resulting values of the coefficients will not be so exact. Observations on 8 points will give less reliable values and observations on the four cardinal points will not give a value of D nor those on the quadrantal points, a value of E.

As D is more important than E, if observations on four points only should be possible in any case, they should be made on the quadrantal points.

It is obvious that having the values of the coefficients we may compute the deviations on any course to half or quarter points by substituting in each equation the azimuth of the course $f\,Z$.

40. These coefficients are, strictly speaking, only approximately correct, the more accurate values being expressed by $\mathfrak{A}, \mathfrak{B}, \mathfrak{C}, \mathfrak{D}, \mathfrak{E}$, which are attained as follows :

$\mathfrak{A}$ = sin. A

$\mathfrak{B}$ = sin. B[1 + ½ sin. D + $\frac{1}{12}$ ver. sin. B − ¼ ver. sin. C] + ½ sin. C sin E

$\mathfrak{C}$ = sin. C[1 − ½ sin. D + $\frac{1}{12}$ ver. sin. C − ¼ ver. sin. B] + ½ sin. B sin. E

$\mathfrak{D}$ = sin. D[1 + ⅛ ver. sin. D]

$\mathfrak{E}$ = sin. E

For all ordinary purposes, however, the values represented by A, B, C, D, and E are sufficiently close.

41. The deviations may be observed by other methods than that of swinging the ship on known bearings. If in dock, or in a confined harbor, a compass carried around ship on shore, by an observer who keeps always in a line with the masts will, at each moment, give the true magnetic bearing of the head of the ship; and she can be stopped on any desired point, or observations of her compass be made as she reaches each point in succession, by means of signals agreed upon by those on shore and on board.

Or the shore compass being kept in one spot, reciprocal bearings of the shore compass, and the ship's compass, can be made in each position of the ship which will agree if there is no deviation, and will differ by the amount of deviation on each course. In using this method, it has been suggested that the readings in each case should be chalked upon a blackboard to be seen by the opposite observers, in order that they may be certain that the readings and records are correct, before making each new observation.

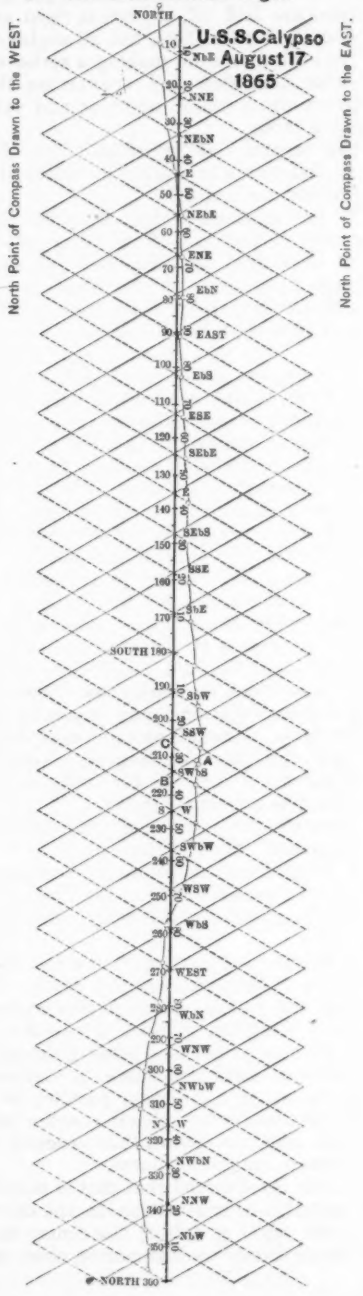
In the case where the standard compass, having been placed in a carefully selected position (49), is perfectly corrected, it will be used as the ship swings to determine the deviations of the other compasses in their different positions on board.

42. The deviations in different courses may be plotted by laying down a straight line to represent no deviation and dividing it equally into thirty-two divisions, representing the points beginning at N at the top, and ending with N at the bottom, (Fig. 4). The amount of deviation on any given course may then be represented by laying off a distance to any convenient scale, equal to the amount of deviation opposite to the point representing the course, and to the right or left as the deviation is E or W.

By laying off a number of these for different courses, and joining the points thus obtained, we shall have a curve of deviations which addresses itself to the eye.

In order to do this in a way which will make a diagram of the greatest service, Mr. Napier, of Glasgow, suggested

NAPIERS CURVE Fig.6



what is known as Napier's diagram, in which the ordinates, on which the deviations are laid off, are not at right angles to the central line, but at angles of 60° in each direction, making a series of isosceles triangles, and converting the diagram into a simple addition and subtraction table.

The central line is divided into degrees and serves as a scale by which the deviations are laid off.

To use it, observe the deviation on any course, and lay off the amount of that deviation on the line passing through the course, on the plain line if the course is correct magnetic, on the dotted line if it be the disturbed compass.

If this course is not exactly on a point lay off the deviation on a line parallel to that passing through the nearest point as A. Having plotted in this way all the observed deviations, a curve is to be traced through them with a free hand, and the deviations on courses not observed upon are given by the intersection of this curve with the dotted or plain lines corresponding to each point.

The examination of a collection of curves, made from actual observations, as in the report of the Liverpool Compass Committee, &c., will show that there is so much regularity that these interpolated deviations may generally be relied upon, although certain cases, such as the U. S.S. Roanoke (Report of National Academy of Sciences for 1863) the irregularities are considerable.

There are two principal practical applications of this diagram.

I. From a given course by compass, which has been steered, to lay down the direct magnetic course which the ship has made.

Find the compass course on the central line. Move the pencil along the dotted line, passing through this course, or in a direction parallel to the *dotted* line, if there is not one passing through the course, until the curve is reached, then return in a direction parallel to a plain line until the pencil reaches the central line, the point thus reached is the correct magnetic course.

It will be observed that this is merely the addition or subtraction, as the curve is to the right or left of the center line of the deviation on this course, since the three sides of the triangle A, B, C, passed over by the pencil are all equal.

II. From a given correct magnetic course to find the course that must be steered by compass in order that the ship may follow the given course.

Find the correct magnetic course on the central line. Move the pencil in a line parallel to the *plain* lines, until it reaches the curve, and back parallel to the dotted lines to the central line, and the point thus reached is the course which must be steered. This is evidently the converse of the preceding operation, and consists in subtracting the deviation which before was added or *vice versa*.

The following verses are given in the English Admiralty Manual to aid the memory.

I.

"From compass course, magnetic course to gain,
Depart by dotted and return by plain."

II.

"But if you wish to steer a course allotted,
Take plain from chart and keep her head on dotted."

43. Having determined the deviations on the correct magnetic courses, it will be very easy to construct a table giving the course to be steered by compass, in order to shape any desired course. It may have the following form:

STEERING TABLE. BINNACLE COMPASS, &c., &c.

Course to be made.	Course to be steered by compass.
N	N 8° W or N ¾ W nearly.
N by E	N 2½° E or N ¼ E "
N N E	N 13° E or N by E ¼ E "
N E by N	N 23° E or N N E "
N E	N 33° E or N E by N "
N E by E	N 43° E or N E ¼ N "
E N E	N 52° E or N E ½ E "
E by N	N 61° E or E N E ½ N "
&c.	&c.
&c.	&c.

44. As a table of deviations is troublesome to use, and as errors may be made in applying the correction indicated by the table, it is convenient to make a corrected compass card to steer by.

This may be done in several ways, first: by determining the deviation by swinging, and interpolating these points not observed on from Napier's curve, and then making a card to be put on top of

the regular compass card, so distorted that the deviation will be allowed for by steering the ship by the new card. Thus, if the deviation when heading E true magnetic, is one point to the E, the west point of the new card must be one point to the left of the E point of the old card or over the point E by N, and so on all around so that the divisions of the card will not be equal, $11°\frac{1}{4}$ to a point, but will be greater or less as the deviation is greater or less in each position. If on N courses there is no deviation the N points of the old and new cards will coincide.

Instead of covering up the old card with the new one, the new points may be marked in red ink on the margin of the card outside of the regular division. (See Towson's Practical Information for Figure, page 29).

In the form of deviation card, devised by Com. Rodgers, U. S. N., the proper magnetic points are drawn on the outside of the compass, and with the arrow heads indicating them pointing towards the center to distinguish them from the old points.

45. A method of plotting the action of the ship on the needle from values of $\mathfrak{A}$, $\mathfrak{B}$, $\mathfrak{C}$, $\mathfrak{D}$, $\mathfrak{E}$, and $\mathfrak{F}$, too elaborate to be inserted here, is given in the Admiralty Compass Manual, under the head of Dygogram, Nos. 1 and 2.

These drawings give the means of studying the action of the various coefficients, and No. 2 may be used as a steering card.

46. It is possible to correct mechanically the compass of an iron vessel in such a manner that the deviations on all courses may be very much reduced, or even destroyed altogether.

Neglecting for the present, the vertical component (25), the action of the ship on the needle may be represented by that of a large magnet N S Fig. 7, or still further resolved into the action of two magnets S′N′, and S″N″ at right angles to each other, one in the midship line, the other athwart-ship. The action of the first may be corrected by a small magnet N^2S^2, placed fore and aft., and near to the compass with poles opposite to those of N′S′ and the action of the second by a small magnet N^3S^3 athwartship with poles also opposite to those of S″N″. In practice this is done by placing the vessel head N, and laying the magnet N^3S^3 upon the deck, either forward or aft. of the compass, turning it end for end or moving it backwards or forwards until the needle points N when S″N″ is corrected. Then heading the vessel to the E or W, the magnet N^2S^2 is placed by trial in such a position that it makes the needle point N when N″S″ is corrected.

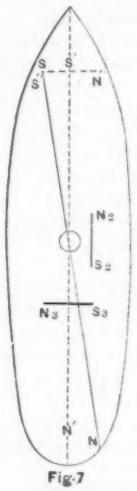


Fig. 7

The magnets are then secured by letting them into the deck and nailing pieces of sheet copper over them.

If the magnetic axis of the vessel coincides with its midship line, no athwart ship magnet N^3S^3 will be required, and it is evident also that if the direction of NS is known only, one magnet N^4S^4, *parallel to it* need be used, but as in practical work this is rarely known the two magnets are used.*

The semi-circular deviation due to the permanent magnetism is thus disposed of, and the quadrantal (30) must be corrected by soft iron placed in a position to counteract the action of the soft iron of the ship. This may be done by having two small boxes, called chain boxes fixed one on each side of the compass, to starboard and port, into which soft iron chain, or pieces of annealed wire or iron are put in sufficient quantity by trial to make the compass point to the N cor-

* This method of correction by permanent magnets is known as Airy's method, it having been proposed by the Astronomer Royal of England.

rectly, when the ship's head is N E. S E. S W. or N W., the correction by the magnets having previously been made. The Liverpool Compass Committee use soft cast iron cylinders with hemispherical ends for the same purpose instead of chain, fixing them to port and starboard of the compass, and moving them farther or nearer to produce the desired effect.

This soft iron thus applied corrects D. E which is due to soft iron unsymmetrically distributed, may be corrected by moving the soft iron either backwards or forwards, but it is usually very small and may be neglected. The magnets used for compensating should be of good steel, made very hard and well magnetized sometime (some months if possible) before being used, so as to acquire a permanent condition of magnetism. They may be from fourteen inches to two feet in length and about $\frac{1}{4}$ in. by $1\frac{1}{2}$ in. cross-section.

It frequently happens that one correction will interfere with one already made, and it is therefore necessary to repeat the adjustments, with the head in different positions, so as to eliminate the errors, or if any obstinacy remain, they must be averaged on the different courses, and noted in the deviation-table or on the card.

In other words, the compasses may not be *perfectly* corrected, but the deviations will be much reduced, and may be allowed for or not according to the judgment of the officer.

The heeling error may be compensated by a vertical magnet (36).

Experiments, for instruction and practice, on the action of the iron in a vessel and the method of correction may be readily made on shore or on board ship in a harbor, by arranging a table capable of being rotated around a centre, to represent the ship, and placing upon it permanent magnets and pieces of soft iron in different positions, to represent the iron of the vessel, a pocket-compass or a small prismatic compass being used as the ship's compass. For instance, in Fig. 7, N S may be a steel wire or rod magnetized and fastened to the table, the compass placed on the middle of this, supported on a couple of books, will show the semicircular deviation as the table is turned into various azimuths.

This may be then corrected by the use of two small steel magnets N^1 S^1 and N^2 S^2 placed by trial in the proper positions.

The soft iron may be represented by pieces of iron rod a few inches long and a quarter of an inch in diameter, which have been heated to a bright red and allowed to cool with the fire or cooled in sand slowly. These may be fixed vertically, horizontally, or inclined, and their action in different azimuths noted.

All the phenomena which occur on iron vessels may thus be studied with great ease, and the practice serves to familiarize the officer with the effects which he will meet in practice, and enable him frequently to refer them to the proper cause.

47. The relative advantages of corrected and uncorrected compasses, the latter to be used with a deviation card, table or diagram, have been frequently discussed.

In the English navy, the compasses being usually uncorrected while in the English merchant service, Airy's Method is generally employed. Nearly all the iron blockade-runners captured during the rebellion, had permanent magnets, which were generally torn up by the captains before capture, with a view to make the navigation difficult to the captors.

Most of the iron and many of the wooden steamers of the United States navy have their compasses mechanically corrected.

It has been urged that the permanent magnets may lose their force, and thus introduce an error, which being unexpected might be serious, but as Mr. Towson remarks, no instance has yet been reported in which the magnets lost their charge more rapidly than the ship lost its magnetism. The heeling error, which can hardly be allowed for in practice by the use of a table, and which is sometimes very large, is much reduced by the compensation, and the effects of rolling on the needle are also much diminished.

As the deviation due to the permanent magnetism depends upon the relation between that and the earth's force, in an uncompensated compass it will change with change of magnetic latitude, and a table must be made for new positions.

Altogether the evidence seems to be strongly in favor of compensation, and in fact some of the recently-built English war-vessels have been obliged to come to it, owing to their very large deviations.

The vigilance of the commander must not, however, be relaxed because the compasses are supposed to be corrected, but every opportunity should be embraced to determine whether they have undergone any change.

It is no doubt very desirable to have an uncompensated compass fixed in the best part of the ship (49) to refer to from time to time. If the position selected is one which cannot be constantly occupied by this compass, it may be so arranged that it can at any time be replaced accurately in position by means of marks on the deck.

48. In order that a compass may be good, the needle should be very hard and well magnetized so as to retain its power, the cap should be of ruby or agate, carefully hollowed so as to be even and smooth, and the point should be hard, fine and sharp.

No dirt or dust should be permitted to get into the cap, as it will make the needle sluggish, and enable the point to grind into the cap. The point should be examined from time to time and kept sharp. It is important also that the point should be exactly in the intersection of the two diameters passing through the gimbals, and that it should be exactly at the same height as the centres of the gimbals, a matter frequently neglected by the maker. It has been shown that a compass is more steady, and that the quadrantal correction is more perfect when the card has two parallel needles, the ends of which intersect the circumference of the card at points 60° apart. The Admiralty compass has four needles.

If the bowl is of copper, or better if a stout copper ring surrounds the card, the vibrations of the needle will be calmed, that is their amplitude will be reduced, while the time of vibration will remain the same,owing to an action being set up which appears to be due to currents generated by the relative motion of the needle and the copper. It may be suggested that no means of *cutting off* the action of the ship's iron from the compass can be effectual, since anything which will do that will also cut off the action of the earth's magnetism also, and render the compass useless.

Owing to the peculiar difficulties experienced in finding proper positions for standard compasses on the Monitors, and to the necessity for having the steering compass in or over the steering turret and, therefore, in a position to be affected by the iron of the turret and of the large guns, particularly troublesome because they change their positions with reference to the other iron of the ship when they turn, several methods have been suggested by which the compass could be raised far enough above the turret to be free from the action of the ship and still to be available for the purpose of steering.

Ritchie's Monitor Compass, the first one suggested for this purpose consists essentially of a needle which works in a copper globe supported at a height of six or seven feet above the turret. This needle is attached to and turns with a delicate vertical axis which is long enough to go down through the top of the steering turret, and which carries on its lower end the compass card, so that when the needle above turns in obedience to the earth's magnetic force the card below turns with it and can be consulted by the steersman. In order to prevent friction on its bearing, and strains on the rod, the whole floats in a liquid the specific gravity of which is the same as that of the rod and its floats and card.

A description of its details which are very ingenious will be found in report of Nat. Acad. of Sciences for 1863.

Mr. Schott, of the Coast Survey, has suggested a compass with the under side of the card divided, like a tell-tale compass placed in a copper globe or box above the turret, and read by means of a mirror placed underneath it, with the aid of a glass if necessary, thus dispensing with the vertical axis and simplifying the apparatus.

Report Nat. Acad. Sc., 1863.

Mr. Hilgard, of the Coast Survey, has devised a Magic Lantern Compass, in which the needle raised as before carries a transparent card of glass or mica, an image of which, by a proper arrangement of lenses, is thrown upon a table in the dark steering turret in front of the

steersman. The light comes from the sky in daytime, and from a lamp at night. In this form a very short needle with its accompanying advantages, may be used.

49. The ship's standard compass, which should be an azimuth compass, should be placed in a selected position in the midship line where the deviations are the smallest and most regular. In wooden steamers, although there may be large deviations at the steering compass, it will usually be possible to find some place where the needle is not disturbed, but in iron vessels this can hardly ever be the case.

It is, perhaps, desirable that the standard compass should not be mechanically compensated, and if it is not, a table and curve of deviation must be prepared for it and every opportunity taken to verify it when the magnetic latitude is much changed. If upon calculating the coefficients (38) for the standard compass a large + D is observed, it may be reduced by placing the compass over a hatchway or a sky light, where the transverse deck beams being divided will act as two pieces of soft iron athwart ships, and thus partially correct the quadrantal deviation.

50. The magnetic condition of parts of a ship can be determined by bringing a small needle near to the part to be examined. If the N end of the needle is attracted, the part is said to have S polarity, and vice versa.

It will be found in examining iron ships, that they have almost, without exception, N polarity at one end and S at the other, and that the line dividing the different kinds, or the line of no polarity, runs obliquely from the bilge to the rail on either side, or that the surface of no deviation coincides more or less closely with a plane passing through the vessel obliquely. Observations on polarity are important, as indicating in many cases the action which the vessel may be expected to have upon the compass.

51. The instruments required for investigations in the ship's magnetism are, 1. A good azimuth compass. 2. The ordinary ship's compass. 3. Boat compasses, which are very convenient for moving about. 4. The ordinary prismatic compass (Schmalkelder's). 5. A small pocket-compass for testing polarity (50). 6. A needle delicately suspended for vibration experiments. (The prismatic compass, if a fine one, will do for this purpose.) 7. A needle balanced so as to hang in the dip, very delicately hung on points or in jewelled holes for vertical vibration experiments. 8. Two or three small magnets, a small level, and a clinometer of some kind, to determine the angle of keel.

52. The most important books and articles on the subject of the magnetism of iron vessels are as follows:

"Admiralty Manual, for ascertaining and applying the deviation of the compass caused by the iron in a ship by F. J. Evans and Archibald Smith, London: Potter, 1863. 4s. 6d. 8vo, pp. 166, plates."

"First and Second Reports of Liverpool Compass Committee. London, 1857. Small folio, pp. 74, plates."

"Third Report of Liverpool Compass Committee. London, 1862. Small fol., pp. 131, plates; 8 shillings."

"Practical Information on the Deviations of the Compass for the use of Masters and Mates of Iron Ships. By Thomas Towson, Esq. London: Potter, 1863. 8vo, pp. 122; 3 shillings."

"Paper by Edw. J. Johnson, Commander R. N., on Magnetic Experiments on an Iron Vessel, in Philosophical Trans. Part II., 1836."

"Papers by Geo. B. Airy, Astr. Royal in Phil. Trans. 1839. p. 167–213, 1856. Part I., p. 53–99. Athanaseum Ap.,1865."

"Paper by Mr. Evans, R. N., in Phil. Trans. 1860. Part II., p. 337–378, and in Jour. Royal United Service Inst. Vol. IX., pp. 22."

"Papers by Evans and Smith, Phil., Trans., 1861, Pt. I, p. 161–181. Phil. Trans. 1865, Pt. I, p. 263–323."

"Report of Compass Committee in Report of Nat. Acad. of Sciences to Congress for 1863."

The principal investigators in this especial subject have been Capt. Flinders, 1801–1803, Dr. Scoresby, 1819, and later, Major Gen. Sabine, Royal Artillery, Dr. Young, Mr. Barlow, Mr. Poisson, Mr. Airy, Comd. Evans, Mr. Archibald Smith, Capt. Johnson, Mr. Randell, Mr. Towson.

In France it does not appear to have attracted the attention of scientific men to any great extent, and in this country

owing to the small number of iron vessels built before the war, it is only within the last few years that it has begun to receive the attention which its importance demands.

It is just now advanced to that point where every officer commanding, or on board of an iron vessel, may add his share to perfecting the general knowledge of the subject, by well devised and carefully executed experiments and observations.

GUNS AND ARMOR.

From "The Engineer."

THE trial of the 38-ton gun, subsequent to its being chambered, is probably the last of the remarkable series of experiments on the power of very heavy guns against armor which has been recently carried out in this country and in Italy. We can hardly let the occasion pass without making a few remarks as to the lessons to be learnt from these experiments. The Spezia trials had for their object not only the proof of the gun's powers, but also the investigation of the kind of material and disposition of plates best suited to resist the fire of artillery. The proof of the gun was conclusive. The penetration, as calculated, agreed very well with that obtained, allowing for the velocity left still in the shot after passing through the target. The target's powers, however, to many may constitute the more interesting element in the trial.

Great fault has been found with this part of the programme. Our readers may remember that there were four targets, each consisting of an upper and lower portion, so that we may speak of them as eight targets. All had an inner skin of 1½ inch of iron, and all had 29½ inches of wood. As to front plates, two had solid Schneider steel plates 22 inches thick, two had similar plates made by Cammell and Marrel, of wrought iron. The remaining four were "sandwich" targets. The 22 inches of plate were adhered to in each case, but in two wrought iron was used, divided into 12 inch plates in front and 10 inch plates behind one layer of wood. In the other two, chilled iron 14 inches thick was employed behind 8 inch plates in front, in one case a layer of wood being between. Our Royal Engineers find great fault with the trial, because they maintain that the half-targets, being only 4 feet 4 inches wide, the plates were broken across by the hole made by the shot of a 17 inch gun. This was the case, as they maintain, even when the shot struck the center, but they point out that the conditions were generally still more unfair on the plate, for the shot often struck near the edge, when a corner would be wedged off and detached much more easily than fair penetration would be effected.

Thus, they urge, the trials were spoilt to such an extent that it is difficult to know what importance to attach to them. We have put the objection perhaps more strongly than it may generally be urged, but it is better to do so in order to see the full force of it; and we are bound to admit that the plates did break across, and did detach themselves, and that, if the plate yielded by breaking or splitting so often, it is clear that the shot found less resistance than in fair penetration. Still we think the Italians may fall back on the fact that their object was to test the plates they meant to put on their own ships. We believe these plates represented the actual dimensions to be used, and if so, the object of the authorities was answered. They did not seek to establish the abstract question of the penetration of a shot into 22 inches of iron of unlimited area, but to acquire information as to how their own plates would stand against the gun. Failure by breaking was as fatal and as necessary to investigate as if the shot penetrated. In fact, they followed the sound practical plan of approaching service conditions as far as possible. We believe, then, that in the main the experiment was well carried out. Nevertheless, the objection made by our Royal Engineers is good to a certain extent. The 22 inch plates, it may be urged, not only were the size in-

tended for the Duilio, but also could only have been made broader by shortening them, so that there is not much to object to in them, but with the thinner plates the question is different. They might have been made wider, and would be made wider, or ought to be so, on their vessels. The reason why our own Government have decided on having two layers of plates on our own Inflexible is because we thereby get a complete plate from top to bottom of our turret, instead of a jointed or divided one. No one would advocate the use of 10 inch and 12 inch plates rather than a 22 inch plate of the same width. In this particular, then, we think a mistake was made. Chilled cast iron cannot be said to have been tried in the way advocated in this country, or on the Continent, as far as we are aware, at Spezia, because no one, not even Herr Grüson, advocates using the same thickness of cast iron as of wrought iron or steel.

Herr Grüson urges that a certain mass of it acts like an anvil to hammer in absorbing the blow delivered, by transmitting it throughout its mass, and he appears to have obtained some remarkable results. He would probably not expect much from 14-inch of chilled cast iron. Nor, as a general principle, would he advocate the use of wrought iron in front, for he values the hard surface as suited to throw off the point which the wrought iron surface rather clings to, and even turns in to itself under certain circumstances, when it might have glanced off a harder material. Commander Grenfell, who put forward the advantages he expected to accompany the use of chilled iron in large masses, at the Institution of Naval Architects last week, scouted the application of it in the Spezia targets. The Italian authorities may have had in view the comparative trial of its powers under these particular conditions; but we hardly think that much came of it. One thing seems clear: by the breaking up of their plates the Italian authorities appear to have been guided in the conclusion to which we hear they have now arrived, which is to make thick steel plates with rather thin wrought iron in front. The wrought-iron surface is probably not the most desirable one, but an outside layer of wrought iron of a fair thickness, and rolled in one single unjointed plate might be expected to hold any broken pieces of steel plate beneath it in their position instead of allowing them to detach themselves. It is easy to see that a turret protected by a perforated plate of wrought iron covering thick steel plates, however much split, is in a very different condition from one from which the plates are wholly or partly detached, reducing it to a condition of ruin, confusion, and exposure.

Chilled shot may be and generally are split longitudinally in their passage through thick iron armor, but they go on capitally as long as the pieces are held in their relative positions. Steel plates might act fairly, as proposed, especially on a turret where the arched form of section might assist them. We should think care ought to be taken that the outside covering of wrought iron was bolted by a certain number of bolts that were not in immediate contact with the steel which transmits the blow throughout its mass, and breaks and injures them much more than wrought iron. On this principle then we should have an outside covering of wrought iron to act as a binder. It ought to be thoroughly soft and tenacious, and have as few joints as possible. Inside we should have steel which would transmit the blow through its mass, and would be liable to be split, but its fragments would not become detached. We should be curious to see structures of this kind severely tested. In fact, we cannot doubt that the Italian experiments have added a great deal to our knowledge of the behaviour of armor, and may bring out very good designs, though it is premature to offer a decided opinion yet.

Our Shoeburyness trials are less interesting. Their scope has been much narrower, because we have not made any such extraordinary special series as the Italians. The novelty in our case has been chiefly the magnitude of the scale on which we have now made guns and armor. One or two new principles, however, have been brought out. The 80-ton gun has done well, decidedly better than could have been expected. It has already penetrated 26½ inches of iron, but the specially important feature brought out in our experiments, is the advantage gained by enlarging the powder chamber. We have gained 200

feet per second in velocity in the case of the 38-ton gun under decidedly unfavorable circumstances, and more is to be expected. The abolition of studs by the base-cup rifling system, which has been adopted and proved so successful in the Elswick guns, must sooner or later come in with us; in fact we are making experiments on the subject now. This ought to give a great advantage to the shot. As mentioned above, our projectiles split longitudinally during penetration. This may be in part due to the stud-holes and studs. We should also hope some experiments may be tried by our Government with the sharp ogival point of $1\frac{3}{4}$ calibres, which is used everywhere abroad, and has been adopted at Elswick.

Lastly, as connected with our recent experiments, we have to notice the terrible effect of common shell on thin armor. A 10 inch Palliser projectile would probably penetrate 12 inches of iron well, but its common shell will penetrate 5 inches of unbacked iron easily, and would produce fearful effects on all behind it, bursting just after it got through, if we may judge from the results of the last experiments. This looks ominous for the Shannon class of partially armored ships, and increases the difficulties to be overcome by our naval architects. In the present state of guns and armor everything is uncertain, and each experiment is likely to reveal some new feature.

BEAMS OF UNIFORM STRENGTH.

By DE VOLSON WOOD, C. E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE so-called "Unsolved Problems," referred to in Professor Robinson's article in the March number of this Magazine, have now been solved in one way or another. In an article published by the writer in the *Journal of the Franklin Institute*, 1862, vol. 43, page 109, will be found the several problems pertaining to beams fixed at one end, the other end being free, in which the weight of the beam constituted a part of the load. But the difficulty of finding the complete integral of some of the expressions was the same as in Professor Robinson's article. Afterwards, however, I discovered how some of them might be integrated (see *Jour. Frank. Inst.*, 1864, vol. 47, p. 28), and I will apply this method to one of the cases which the Professor rejected as non-integrable by ordinary methods, although I doubt if it will add anything to our knowledge in regard to the form of the beam. The curves represented by the equations can be classified only as *transcendental*.

The solution given by Professor Robinson, of the problem pertaining to beams supported at their ends, the beams forming a part of the load, is the only one I have seen.

The other problems referred to (*Wood's Resistance of Materials*, p. 197), have been solved by means of the *Principles of the Elasticity of Solids*, formulas for the results of which are given in the *Scientific American*, Oct. 22, 1875 (see also *Grashof's Festigkeitslehre*, Berlin, 1866); but the original intention was to have them solved in accordance with the principles of *Ultimate Strength*. We are not, however, informed of any solution by the latter method. One of the general values given by Professor Robinson is:

$$x=\sqrt{\frac{12}{\mathrm{R}}}\int\sqrt{\left(\frac{3\mathrm{P}^2}{\mathrm{R}}+wby^2+\tfrac{2}{3}\delta b^2y^3\right)};$$

in which b is the breadth of the beam, and is constant, δ the weight of a unit of volume of the beam, w the weight per unit of length of the uniform load, P the weight at the free end, R the modulus of rupture, x horizontal, and y vertical.

Let $w=0$, and we have:

$$x=\sqrt{\frac{2\delta}{\mathrm{R}}}\int\sqrt{\left(\frac{9\mathrm{P}^2}{2\delta b^2\mathrm{R}}+y^3\right)}$$

which was supposed to be non-integrable. Let

$$y=\left(\frac{9\mathrm{P}^2}{2\delta b^2\mathrm{P}}\right)^{\frac{1}{3}}(z^3-1);$$

then

$$dy=2\left(\frac{9P^2}{2\delta b^3 R}\right)^{\frac{1}{4}} z\,dz$$

and the equation becomes

$$x=\left(\frac{6PR}{b\delta^2}\right)^{\frac{1}{4}}\int\frac{(z^2-1)\,dz}{\sqrt{[3-3z^2+z^4]}}.$$

This comes under a general formula, given in Legendre's *Elliptique Fonctions*, which is written as follows:

$$X=\int\frac{(f+gy^2)dy}{\sqrt{[a^2+2aby^2\cos.\theta+b^2y^4]}}$$

$$=\frac{fb+ga}{b\sqrt{ab}}\,F(c,\theta)-\frac{2ga}{b\sqrt{ab}}\,E(c,\theta).$$

Hence our equation becomes

$$x=\left(\frac{6\,P\,R}{b\delta^2}\right)^{\frac{1}{4}}$$

$$\left\{\frac{\sqrt{3}-1}{\sqrt[4]{3}}F(c,\theta)-2\sqrt[4]{3}E(c,\theta)\right\}$$

In this equation $c=\frac{1}{2}\sqrt{2+\sqrt{3}}$.

[Since I wrote the above, I have attempted to verify Legendre's integral, and find that there is something erroneous in it, so that I am not certain that the expression can be integrated even by Elliptic Functions. I trust that his *result* will be found to be correct.]

REPORTS OF ENGINEERING SOCIETIES.

THE AMERICAN SOCIETY OF CIVIL ENGINEERS—NINTH ANNUAL CONVENTION.—At 10 o'clock A. M. of Tuesday, April 24, the Convention opened at the Chamber of Commerce, in New Orleans; the opening address being delivered by Col. W. Milnor Roberts, in which he reviewed the history and growth of the Society, and spoke in eulogistic terms of the great engineering work in progress at the mouth of the Mississippi. Prof. Forshey, of New Orleans, well known as the hydraulic engineer, was elected Chairman of the Convention. Eighty-five members, associates and guests were found to be in attendance. The first paper read and discussed was that of Charles Macdonald, C. E., on the Ashtabula bridge. It was generally agreed that the building and inspection of the bridge were grossly defective. A paper on the same subject was read by Mr. E. S. Philbrick, of Boston. The remedy proposed was official inspection by men entirely separate from corporate interest, either appointed by the general or State Government, and a recommendation that a committee of four from the Society be appointed to draft a law, and that individual members interest themselves in procuring its passage in the various States, so as to secure practical uniformity in its workings.

At the afternoon session, a paper on the relative quantity of material in girders of different types was read by Mr. Charles Emery, of New York, and discussed at considerable length. Papers were also presented by Major Howell, on the Improvement of Galveston Harbor; and by the Secretary, for Gen. J. G. Ellis, on the Flow of Water in Rivers. Mr. E. L. Corthell, engineer in charge of construction of the Eads jetties, made some valuable remarks on the progress and problems of that work. He acknowledged that they had been over-confident, and had made some mistakes, but said that the results thus far had been most encouraging, and ensured final success. The chief point discussed was the determination of the slope of the surface of the Mississippi from Carrollton to the Gulf. Mr. W. H. Searles, of New York, read a statement, giving the results of recent delicate test levels upon the line of the Erie Canal, showing more accurate work than had been accomplished by engineers in foreign countries. A paper by Mr. S. Toster Flagg, of Meadville, Pa., upon the insufficiency of steam vacuum pumps, was read by the author, and discussed by Mr. C. E. Emery, and by Mr. W. E. Worthen, of New York.

At the evening session a paper from the late Col. G. W. R. Bayley, on the levees and the river, a criticism on a criticism made by Gen. Warren on a paper of Mr. Bayley, was read.

A report of a committee on the metric system of weights and measures, recommending the adoption of the system was read by Mr. Clemens Herschel, of Boston, Mass., the chairman of the committee. Mr. Philbrick, of Boston, presented a resolution recommending a committee of five to be appointed to confer with other bodies on that subject. A paper by Col. W. M. Roberts, on the American Society of Civil Engineers and its Future, was also read and received with marked approbation by the members of the Convention. Mr. James B. Francis, of Massachusetts, was then called to the chair by Prof. Forshey, who delivered a eulogy on the late Col. G. W. R. Bailey, which was accepted and spread on the minutes of the proceedings.

At the Wednesday morning session, Gen. W. Sooy Smith, chairman of the committee to whom, at the last annual meeting, was referred the question of tests of iron and steel, made a lengthy report, whereupon, on motion, a committee of three was appointed to draft resolutions expressing the thanks of the Convention for the able report, and to suggest a proper method to bring the subject before Congress. The committee reported following resolutions, which were unanimously adopted;

WHEREAS, In 1872 a committee was appointed of members of the American Society of Civil Engineers, to take into account and to ascertain the best way of establishing a Board for the testing of such metals and alloys thereof, as form parts of such structures and machines as are required for the use of this country; and,

WHEREAS, In pursuance of such appointment the committee proceeded in their labors so far as to obtain favorable action from the Congress of the United States, not only in a law authorizing the creation of a Board for the purpose of making such tests, but also appropriated money to be expended by said Board in the purchase of the necessary machinery, and in the making of such tests; and,

WHEREAS, At a late session of Congress a law was passed by which said Board would cease to exist on the expenditure of the money then appropriated; Now, therefore,

Be it Resolved, That this Society deem the tests proposed to be made of National importance; they, therefore, ask that so much of the sundry civil appropriation bill passed by the Congress, as provides that the Board to test iron, steel, and other metals, shall be discontinued when the money appropriated for its use by the same shall have been expended, be repealed, and the unexpended balance in the hands of the Board shall be re-appropriated, and such further appropriations be made for the use of the Board as it may require to complete the investigation it has undertaken, the sum required for the coming year being $40,000.

Resolved, That every member of this Society be urged to use such influence as they may possess, to obtain favorable and immediate action by the Congress of the United States, in furtherance of the object here prayed for.

Resolved, That the above resolutions be printed, and that each member of this Society be furnished with several copies thereof, to be used in furtherance of the object sought, and that reports of their action in the premises shall be sent by the member to the Secretary, giving the names of such members of Congress as have been seen or addressed on the subject.

JOHN GRIFFIN,
G. BOUSCAREN,
EDWARD S. PHILBRICK,
Committee.

At the evening session Prof. Forshey read a paper recommending certain radical changes in our patent laws, and describing some inventions of his own—especially an irresistible brake, automatic switch, and "express snatch." Col. Hardee presented and read a paper giving his experience in a hurried survey of the railroad from Jackson, Miss., to Jackson, Tenn., in which the lines were run at the rate of nearly thirty-three miles per day. Mr. Bouscaren explained the manner in which the Cincinnati Southern Railroad bridge over the Kentucky River was tested, and giving the vibrations in the bridge and piers, with a blackboard explanation. A resolution of thanks was tendered to all who had extended courtesies to the society, also to the presiding officer, and at 9:30 P. M. the Convention adjourned *sine die*.—*Chicago Railway Review*.

LIVERPOOL POLYTECHNIC SOCIETY.—At a meeting of this society held at the Royal Institution, Mr. C. H. Beloe, C.E., read a paper on "Engineering in America." The disgraceful state of the street paving and the objectionable mode of construction adopted in the street tramways were referred to. A description was given, and a drawing exhibited of the New York Elevated Railroad, which traverses the streets at the height of the first floor of the houses, being supported on columns placed near the curbstones. After mentioning the great works which were undertaken for the removal of the reef at Hell Gate, the author described the suspension bridge in course of construction over the East River to connect New York with Brooklyn. Mr. Beloe then described the principal features of the Centennial Exhibition at Philadelphia, and alluded to the most interesting of the engineering exhibits. In speaking of the waterworks of Philadelphia, it was mentioned that the average consumption of water in twenty of the principal American cities amounts to no less than sixty gallons per head per day, as much as ninety gallons being the average in one place. In conclusion, the author expressed his astonishment at the progress which has been made in America in the manufacture of iron and steel, and of machinery, and his conviction that the export of these articles from England is already a thing of the past.

IRON AND STEEL NOTES.

SOLID CAST STEEL.—The difficulty encountered in obtaining properly solid cast steel in masses is well understood. The means of overcoming the difficulty have probably yet to be found. Sir J. Whitworth's compressed steel represents one attempt. The system of compression tried some years ago at Neuberg in Styria is another. Yet a third system is now proposed by M. Pourcel, the manager of the steel works at Terrenoire. At the November meeting of the Société de l' Industrie Minerale, a paper was read on the manufacture of steel perfectly free from air cells by M. Pourcel, which involves some very important propositions. M. Pourcel began by saying that from the day that the different phases of the Bessemer process were fully explained, the means of casting steel free from cells was a *a priori* discovered:

"It was well known that silicon prevents the formation of carbonic oxide; here then the principle was established, what remained then was to draw from it all its important consequences, and apply them, and it is from calculations and experiments based on the above law, that the authorities at Terrenoire had been enabled to solve the problem, and achieve their present great success in casting steel free from air cells.

"In decarbonizing a gray silicious pig iron in the Martin furnace by means of wrought iron or steel scrap, we find on taking samples after each addition of steel or iron, that at a given moment the cast metal is riddled with air cells. If, on the other hand, we submit to analysis the homogeneous samples—*eprouvette non soufflee*—taken immediately before the honeycombed sample—*eprouvette soufflee*—we find silicon in the homogeneous sample, whilst the honeycombed metal may contain particles of interposed cinder, but no free silicon.

"Such is the analytical result; the effects may be produced synthetically. By adding silicon in the form of silicide of iron to a bath of steel already formed, we completely eliminate the air cells. This steel is, however, generally red-short, and this red-shortness has been, and is still attributed, not only by steel manufacturers, but also by the majority of our most distinguished chemists, to the presence of silicon. This explanation we admitted in the

first instance reservedly, but now we cannot help thinking that it is open to doubt. Our own opinion is that silicon in the proportions usually found in steel in nowise affects its quality, making it neither red nor cold-short.

"The air cells existing in cast-steel are due—as Bessemer himself demonstrated some eight or ten years ago—to the oxide of carbon formed in the liquid steel by an intermolecular re-action between the carbon of the metal and the oxide of iron formed during the act of casting. When the metal remains liquid sufficiently long, the gas escapes, but generally speaking, the temperature at which steel is cast being only a little higher than that at which it solidifies, the carbonic oxide remains imprisoned, and gives rise to air cells, disposed symmetrically and perpendicularly round the great axis of the ingot.

"Silicon prevents the formation of these air cells—because it has a greater affinity for oxygen than carbon has—and this, of course, by intermolecular combustion, the oxidising body being either peroxide of iron or carbonic acid, or both, but then instead of being gaseous, the product of the oxidation is a solid body formed in the metal itself, and is disposed uniformly between its molecules. It is the silicate of iron, the cinder interposed between the molecules, that renders the metal red-short and diminishes its quality as cast metal.

"To get rid of this cinder we must add to it a base which will render it sufficiently fusible to allow of its separating by liquation. For this purpose we use manganese; and here lies the chief point. Manganese is employed in the Bessemer process to free the cast metal from the peroxide of iron which it holds in solution, it reduces it to its lowest step of oxidation by taking up the equivalent of its oxygen, and the oxide of manganese thus formed, combining with the silicate of iron, renders the cinder extremely fluid and it separates by liquation.

"MM. Troost and Hautefeuille, in their valuable works communicated to the 'Académie des Sciences,' have confirmed this explanation of the part played by the manganese added at the end of the Bessemer process, a theory published by Valton eight years ago in a memoir communicated to the 'Bulletin de l'Industrie Minerale.' Now this theory has entered into practice, and its results have been most fruitful.

"Acting on this law, we have substituted for a silicide of iron a 'double silicate of iron and manganese' for adding to a bath of steel for the purpose of producing metal free from air cells. The two reducing elements, silicon and manganese, act simultaneously on the mass in a state of fusion, reducing the peroxide of iron and preventing the formation of carbonic oxide; and the result of their oxidation is a silicate of protoxide of iron and protoxide of manganese, a body which is very fluid at the temperature at which steel solidifies, and which separates easily by liquation. As to the silicide of iron in excess, we are convinced that its effects are not injurious.

"We cannot here enter into an examination of the experiments on which this theory is based without departing too far from the subject before us, but at one of the next meetings this question will be laid before the members by a person more authorized to speak on the matter. The principle of this manufacture, which we have now discussed, is very simple in theory, but its application is a delicate and more complicated matter. However, we may say that the practical difficulties have in a great measure been surmounted, and at present we are producing at Terrenoire cast steel of nearly every shade characterising forged steel, from the hardest to the softest.

"The perfect homogeneity of these cast steels, resulting from their chemical composition, and the perfect equilibrium of their molecules produced by a process of annealing or tempering of varying nature, will, we firmly believe, lead to results never before attained in the production of forged steels. This is, however, a new subject with which we personally are not so well acquainted, and which M. Euverte, the director of the Terrenoire works, intends submitting for discussion at the next monthly meeting of the society."—*Engineering.*

RAILWAY NOTES.

THE *Gazette de Lausanne* gives the following statistics respecting the working of the railway system of Switzerland during 1875, and which, if not quite recent, contains some useful information. It appears that the total length of the Swiss network at the close of last year was 1454 kilos. (901¼ English miles), of which 214 kilos. was double line (132¾ miles). The average gradient of these lines is 6.83 per thousand (1 in 1610). The mean radius of the curves is 1718 meters (about 86 chains). The capital expended upon these lines is estimated at 515,033,048 f. (£20,701,322), which averages 362,511 f. per kilo. or £23,335 per mile. The rolliug stock consisted of 340 locomotives, 154 tenders, 1037 passenger carriages, capable of containing 46,663 persons; 214 luggage vans, and 5571 good trucks of the capacity of 55,583 tons. With this *materiel* 15,455,157 passengers were carried in the abovementioned year, 2¼ per cent. of which were 1st class, 21 1/10 per cent. 2nd class, and 76 6/10 per cent. 3rd class, besides 3,992,685 tons of goods. The total expense of working these lines amounted to 26,328,313 f. and the receipts to 48,887,658 f., giving a net profit of 22,559,345 f., which represents a dividend of 4.23 per cent. on the capital employed. During the same year 124 persons were wounded and 59 killed by accident. The staff is composed of 6430 employés and 4322 workmen.

FAST TRAINS *vs.* SAFE TRAINS.—We observe that some of our English neighbors have become alarmed by several severe accidents on very fast passenger trains—notably those of the Great Western road, which average fifty-one miles an hour (excluding stops), and on some stretches make sixty miles an hour—and are raising the question whether such speed is safe or profitable. It is announced that since the last great accident to the "Flying Dutchman" train on the great Western it has been

decided to reduce the speed of that train, "over what may be considered the more dangerous part of the road," to forty miles per hour. The Bristol *Times* is so wrought up by the frightful accident referred to that it suggests governmental interference to prevent such great speed—a hint of another form of railroad restriction which our grangers do not seem to have thought of. It quotes approvingly from another writer:

"Why do we (says he) cage in the top of the monument, and fence round the cog wheels of a machine, and require the use of the Davy lamp in mines, if we admit the right of the traveling public, in a preposterous fancy for fast going, to expose themselves and others to the risk of such casualties as that which occurred recently. There is no material known which the tremendous rate of these railroad racers must not severely try, and the cursory tap of the hammer upon the wheels, as they pause for a few minutes at long intervals on their course, are more a seeming than a substantial security. . . . We are quite sure that shareholders would be glad that government took the matter in hand, for they have no difficulty in understanding that the wear and tear of these flying trains upon permanent way, upon locomotives, and upon rolling stock, are such as must greatly interfere with profits. Indeed, it is calculated that a general moderation of speed to forty miles an hour would be attended with an advantage more than equivalent to the abolition of the passenger duty."

Quoting this, *Herapath's Journal* adds:

"An accident such as that to the Flying Dutchman may excuse and justify the interference of government. Safety is of far more importance to travelers than speed, and as railways are the only means of communication between distant points the public have a right to be assured of the greatest safety in traveling, at least that every precaution shall be taken to prevent accidents."

Possibly our British friends will not consider it impertinent for us to suggest, with all respect to the magnificently solid character of British road-beds, superstructure and engines, that with the more strongly built cars, the anti-telescoping platforms, the self-loosening couplers and the automatic air or steam brakes in use on first-class American railroads, such accidents as the one referred to would be almost impossible of occurrence.—*Railway Age.*

ENGINEERING STRUCTURES.

THE SOUTH PASS IMPROVEMENT.—A correspondent of the New Orleans *Times*, under the date of April 29th, writes as follows:

In 1875, the board of civil and military engineers, which reported on the improvement of the mouth of the Mississippi, said: "It is 11,900 feet from the 30 feet curve inside the Pass across the bar to 30 feet outside. The minimum depth on the bar is seven feet." Captain Eads began his work June 14, 1875, less than two years ago, and he now has a 30 foot channel and more through nearly the whole line of the 12,000 feet of jetty channel. There remains just at the lips of the jetties a short shoal with 21½ feet of water on it, where two years ago there were but 7½ feet. This is the only remnant of the old bar left, and it is so short a distance from the deep water inside to the deep water outside, that a large steamship drawing 23 feet might lodge on the ridge of this bar, and both her stem and stern would be in deep water. It is calculated that the spring or June rise will scour out this small obstruction, and leave a channel of full 24 feet, and once gone, it is now certain that it will not return.

No man with scientific knowledge or practical common sense can examine the jetties and doubt this. With high water, there will be at ebb-tide a solid prism of water with an average of 20 feet thick and 800 wide pouring through these jetties and over the bar at a velocity of six to seven miles an hour. It throws a solid volume of water many miles out into the gulf, and no deposit is possible at the mouth of the jetties.

The jetties are protected from the sea by miles of sand bars which have formed behind the jetties along the whole line, and the channel is now like a canal 1000 feet wide cut through sand bars and walled up.

THE fears as to the filling or silting up that were entertained and freely expressed in the most influential quarters, during the early history of the Suez Canal is an event that may now be considered extremely unlikely. Last year between the two seas only 52,700 cubic meters of "stuff" were removed, and the canal was navigated with facility by steamers drawing as much as twenty-seven feet, and over 400 feet in length. The bed of salt which forms the bottom of the Bitter Lakes, is gradually dissolving, so that this portion of the canal is being gradually but steadily improved, and with the increase of vegetation along the banks of the canal there is a prospect of the production, in a not distant future, of a fertile and populous tract of country out of a sandy waste.

THE report by the engineer of the Tay Bridge undertaking states that the whole of the eighty-five piers are placed in position. Seventy of these are completed, having the permanent girders placed thereon, with cross girders and timber platform laid for about 2375 feet, and 3074 feet of hand-railing erected. Of the remainder, five piers are completed to the full height of the string course, and the girders of the spans laid thereon. Two large caissons are sunk to the requisite depth; one is one-third filled with concrete, the other is made ready to be filled. One of the twenty-one feet caissons is thoroughly filled with piles, and ready to receive the concrete foundation; and the last of the twenty-one feet caissons is placed in position in the bed of the river. Neither on the night of the accident during the gale, nor at any other time when even severer gales prevailed than on that night, has the slightest damage accrued to any part of the bridge placed in a permanent position.

ORDNANCE AND NAVAL.

THE BESSEMER.—This vessel has lately undergone considerable alteration. The swinging cabin has been taken out, and the low ends fore and aft built up level with the upper deck. The extra internal capacity thus gained has been appropriated at one end to a second class cabin, and at the other to the accommodation of the officers, crew, &c. The original steering gear has been taken out and Brotherhood's new gear put in in its place. A trial took place off Great Grimsby lately, when it was found that with 130 tons of coal on board the vessel made about fifteen knots an hour. As, however, she was only worked up to twenty-five pounds pressure of steam, doubtless a greater speed will yet be obtained. The new steering gear was found to be rather too powerful, but this Mr. Brotherhood explained could be easily remedied. The ship, however, answered her helm readily. Though not much sea, there was a very considerable swell on, so that the steadiness of the Bessemer was severely tested. She did not, however, roll to any great extent, probably owing to the large bilge keels—two feet six inches in depth—with which she is fitted.

THE THETIS.—Her Majesty's ship Thetis has undergone some peril, and met with an accident of an unusual character. In the navy machinery catastrophes of all kinds abound, and it is to the credit of the ship that she should have given the world something new in this direction. It appears that the Thetis, running home from Port Said in a tolerably crowded marine highway, succeeded in partially getting rid of her screw propeller—that is to say, it came off the shaft and stuck in the screw aperture instead of going straight to the bottom. In the language of rowing men, it became a passenger. This feat accomplished, the Thetis next proceeded to perform one of still more startling dimensions. She so effectually hid herself, that for twenty-five days no one could find her, and as she had started with six days' provisions only, it is clear that during the latter portion of her voyage nothing very eatable was left on board. A whole host of questions rise up at this point and demand answers. Why did the screw come off the shaft? How did the ship conceal herself for twenty-five days in a comparatively populous sea? Why did not the Thetis make for some port under sail? Many more questions might be asked, but we venture to think that if those we have put are repeated in the House of Commons, some good may be done. At all events, we may learn something as to the utility of "masts and spars" in her Majesty's navy and in a breeze.

BOOK NOTICES.

PROCEEDINGS OF THE INSTITUTION OF CIVIL ENGINEERS.

THE SEWAGE QUESTION. By C. NORMAN BAZALGETTE.

ABSTRACTS OF PAPERS IN FOREIGN TRANSACTIONS AND PERIODICALS. Edited by JAMES FORREST, Secy. London: Printed by Wm Clowes and Sons.

In treating the Sewage Question the Author deals with the various systems that have been tried within the past few years, classifying them for the purpose of his discussion as follows:

1. Treatment with Chemicals.
2. Application of Sewage to Land, including irrigation and intermittent downward filtration.
3. Dry Earth System.
4. The Liernier or Pneumatic System.
5. Seaboard and Tidal Outfalls.

The paper and discussion cover nearly 200 pages.

We shall publish most of the essay in an early number of this magazine.

The abstracts are as usual papers of much value to the engineering profession.

A TREATISE ON THE SCIENCE AND PRACTICE OF THE MANUFACTURE AND DISTRIBUTION OF COAL GAS. New York: D. Van Nostrand. Issued in Parts at 40 cts. each. (4 now ready.)

This treatise commences with a short sketch of the history of gas lighting, gives a full account of the geology, lithology and chemistry of the raw material—coal, and afterwards follows the gas, step by step, from the retort to the burner, showing what is done and what is required at every stage of the manufacture and distribution of coal gas. After this naturally follows a brief account of the manufacture of gas from materials other than coal, and some description of the inventions designed to supersede coal gas by the carburation of atmospheric air, and the gases produced by the decomposition of water. Finally, some space will be devoted to an account of the utilization of residuals. In this way it is intended to give a full and complete account of the science and art brought down to the most recent times.

The treatise is published in monthly numbers, royal 4to, and printed in clear, bold type, and each part illustrated with numerous engravings and plans. Its preparation has been confided to careful and experienced editors, and no pains nor expense will be spared to render it complete in every department.

The general plan of the work will be seen from the following divisions:—

Part I, Chap. 1. History of Gas Lighting. Chap. 2. Coal—Geology of Coal; Origin and Formation of Coal; Various Kinds of Coal; Analysis of Coal; Gas Producing Power of Coal; Storage of Coal; Heating power of Coal.

Part II, Chap. 1. Manufacture of Gas. Chap. 2. Retort Houses; Retorts, Iron and Clay; Hydraulic Main. Chap. 3. Condensers and Exhausters. Chap. 4. Scrubbers and Washers. Chap. 5. Purifiers.

Part III, Chap. 1. Chemistry of the Manufacture and Purification of Gas. Chap. 2. Photometry.

Part IV, Chap. 1. Storage. Chap. 2. Station Meters. Chap. 3. Gas Holder Tanks—Brick, Iron, Stone, Concrete, Composite. Chap. 4. Gas Holders. Chap. 5. Pressure Registers. Chap. 6. Designs and Plans for Gas Works.

Part V, Chap. 1. Distribution. Chap. 2. Station Governors. Chap. 3. Mains. Chap. 4. Services. Chap. 5. Valves. Chap. 6. District Governors. Chap. 7. Meters.

Part VI, Chap. 1. Consumption. Chap. 2. Fittings, Burners, Regulators, etc. Chap. 3. Public Lighting.

Part VII, Treatment of Residual Products; The Various Processes of Manufacturing Gas from other Material than Coal, etc., etc.

STRENGTH AND CALCULATIONS OF DIMENSIONS OF IRON AND STEEL CONSTRUCTIONS. Translated from the German of J. J. Weyrauch, Ph. D. New York city: D. Van Nostrand. Price $1.00.

Another translation of this same work has already been briefly noticed in these columns; and we expressed the view that the contents of the volume were not in such practical form as would adapt it to the uses of the working engineer. The present translation seems to us much less open to that objection, and certainly it contains an immense amount of useful data, entirely outside the formulæ, besides examples tending materially to elucidate the latter. The book is rendered much more practical; and its whole arrangement is, to our minds, better and well calculated to render its various topics more accessible to the student. As regards the intrinsic merits of Professor Weyrauch's work, and in our previous strictures on the other translation, we intended no disparaging reflection upon them; they are undoubtedly great, and the volume should be carefully studied by all engineers. It is based on a general view of the results obtained in the extended course of experiments made in Europe and in this country to determine the properties of iron and steel. As these trials have shown the somewhat startling fact that (to quote Professor Weyrauch) "the method hitherto employed in calculating the dimensions of iron and steel constructions have been entirely wrong," it is hardly necessary to point out the importance of any work which deduces a formula which gives all "the requisites for a simple and rational determination of dimensions."—*Scientific American.*

THE ELEMENTS OF ANALYTICAL MECHANICS. By DE VOLSON WOOD, A.M., C.E. New York: John Wiley & Sons. 1876

[From "Engineering and Mining Journal."]

The science of mechanics has a twofold importance. Its practical importance consists in this: that the principles and processes of all mathematical physics are derived from it. It is of the greatest utility in the explanation of the phenomena of the world external to us, and it is indispensable in a thousand applications of science to the needs of life. But its importance in a theoretical point of view is perhaps still more striking. The development of every physical science has been characterized by certain inevitable and successive processes. First of all comes the collection of facts; then the formation of a theory to explain them; then the establishment and verification of this theory by the successful prediction of the future; and, finally, the reduction of the science to its simplest principles and most general form. Through all these processes the science of mechanics has passed, and it has finally attained to a perfection and completeness to which no other physical science can make pretension. It is, therefore, not only the foundation-science to the principle of which every special branch of physics conforms; it is the *type*-science: and the degree of completeness of any physical science is to be determined by the conformity which it exhibits with the principles of mechanics, and the advance which it has made in the process of development which the type-science seems to have well-nigh completed.

Corresponding to this twofold aspect of the science, we have two methods of considering it in its relations to scientific education. First, it may be taught merely with reference to its practical uses. It is not to be denied that in certain applications of the science, as in engineering, this method is quite sufficient, and it is not necessary, for these practical purposes, to consider the theory of mechanics in all its generality. Secondly, it may be studied in its most general form; as it is developed, for instance, in the writings of the great master Lagrange, to whom, perhaps, more than to any other it owes its perfection. This method, however, while it is of incalculable value and importance to any student who wishes to acquire broad scientific views, is lacking in a practical point of view, and in its original form is probably too abstract for most beginners. Fortunately a third way is open in the judicious union of the two methods. This, then, is the problem which presents itself to the writer on analytical mechanics for the purposes of instruction. While he is to take care that the practical character of the science shall be clearly brought out, so that the student shall not find himself provided with tools which he is unable to use, he is also to see to it that the scientific spirit, the perfection and generality of the great type-science are kept distinctly in view. There are indications that Professor Wood has proposed this problem to himself in the book which we review. We cannot, however, compliment him upon his success in its solution.

The most essential requisites of any treatise on mechanics, whether it is practical or theoretical in character, are clearness and accuracy. We will, therefore, proceed first to examine the book with reference to these points. On the very first page we find the following remarkable definition, "Matter is that which receives and transmits force." In point of fact, we have no reason to believe that force is transmitted by matter. Taking the force of gravitation as an example, we have strong reason for believing that it is not transmitted in time or dissipated in space. In a word, Professor Wood defines matter by a property which it is not known to possess. The absurdity of the definition can be made still more apparent by substituting for the word *force* its definition given on the second page. We then find that *matter is that which receives and transmits that which tends to change its state with regard to rest or motion!*

Perhaps it will be well, at the outset, to indicate what is the matter with Professor Wood. A recent article on the subject of Force, which he has contributed to VAN NOSTRAND'S MAGAZINE, reveals the trouble. We do not propose to examine the article in detail. Its errors are

too apparent to escape the notice of any one who has a sound elementary knowledge of the subject. We refer to it only on account of the light which it throws on Professor Wood's errors. "I do not think it advisable," says our author, "to consider anything as force or as the measure of force which cannot itself be measured by this unit [the pound]. Momentum MV is measured in *foot-pounds* [!], and hence *generally* is not a measure of force!" In this quotation we have the whole difficulty. Professor Wood, here and throughout his book, has utterly failed to appreciate the distinction between forces and the effects which they produce.

Within recent years a school of philosophers has sprung up, taking its origin in Germany and France, which has assumed to change the nomenclature of the science of mechanics, and to call that force which, in accordance with the system of Newton, should be energy. We do not propose to discuss the advisability of this change. One effect of it is very apparent: it has confounded many popular scientists, and it seems to have hopelessly confused Professor Wood, for he gives us a definition of force according to one system, and a definition of matter according to another. But this is not all. In order that the student may not have "a vague and confused idea" as to the real meaning of the word force, he is to consider but one kind of force, "that which is equivalent to a pressure." Now, according to the generally accepted meaning of the word pressure (and we cannot suppose any other signification is intended, since no definition is given), it denotes the statical effect of a force, not the force itself. Nor can it be considered, in any true sense, to be equivalent to the corresponding dynamical effect, which is motion, since these magnitudes are referred to entirely different units. According to Professor Wood, however, the magnitude of a force is known only when it is given in pounds; yet he does not altogether reject the dynamical measure of force, although he fails to point out what the unit of measure is. Many other passages in the book seem to show that Professor Wood imagines pressure and motion to be equivalent. For instance, on page 67, "We have seen that a pressure, when acting upon a free body, will produce a certain amount of motion, and that this motion is a measure of the pressure"! But motion cannot be the statical effect of force. What, then, can Professor Wood mean by pressure? Is this notion introduced to prevent the student from getting confused ideas?

Professor Wood's inaccuracies are not confined to the subject of force. For instance, we are told that "density is the mass of a unit of volume"!—an example of the author's prevailing carelessness in confounding the thing with its measure. Again, on page 45, we have the remark, "Every moving body on the surface of the earth does work," etc. Here we have moving matter confounded with working force. Again, on page 58, "One of the grandest generalizations of physical science is, *that no force in nature is lost.*" Here we have force again confounded with energy. Again, on page 89, "A statical couple cannot be equilibrated by a single force. It does not produce translation, but simply rotation." Of course a statical couple does not produce motion of any kind. Again, on page 180, we have central forces defined as "such as act directly towards or from a point called a center." The distinguishing characteristic of a central force—that its intensity is a function of the distance—is ignored. According to the above definition, all forces are central. Again, on pages 203 and 220, we have specific reference made to the principle of D'Alembert, yet this principle, historically one of the most important in the science, is not elsewhere mentioned, and is nowhere stated or explained. Again, on page 59, we find the following: "The terms '*Impulsive force*' and '*Instantaneous force*' are frequently used to denote the effect of an 'Impact'; but since the effect is not a force they are ambiguous," etc. We doubt if any writer of reputation ever used the above terms in the sense indicated; but what are we to think when Professor Wood follows the above remark with this statement?—"An *incessant force* may be considered as the *action* of an infinite number of infinitesimal impulses in a finite time"!

We could cite many more evidences of Professor Wood's inaccuracy and want of clearness, but we shall confine ourselves to a very few. In his treatment of the important subject of the moment of inertia we find no attempt at an explanation of the functions of such a quantity. The only relation between the moment of a force and the moment of inertia of a body with reference to an axis is that they are both measures of capacity, although of different kinds. The one measures the capacity of a force to produce rotation about the axis; the other, the capacity of the body to store up work during such a motion. In place of a clear statement of the use of such quantities in the science, Professor Wood gives a fanciful relation which he thinks he has detected between them. The moment of inertia, he says, may be considered as the *moment of a moment*, whatever that may mean. We need scarcely say that such a motion is useless and confusing, and does not exist in any physical sense.

Again, we find an elaborate description of Kater's method of determining the length of the simple second's pendulum, in which most of the space is devoted to a description of a method of determining the time of oscillation. This description might well have been omitted from an elementary text-book, and the space devoted to a clear explanation of the theory of the apparatus employed. No such explanation is given. The construction and use of Kater's pendulum depends upon two principles. First, the axes of oscillation and suspension are convertible. Second, the time of the oscillation is a minimum when the axis of suspension passes through the principal center of gyration. This second principle is of equal importance with the first; but Prof. Wood ignores it altogether, and misses the point of the method.

Referring to the preface of the book, we find the following remarkable paragraph: "This work, though analytical, is, in a certain sense, the reverse of Legendre's celebrated *Mecanique*

Analytique. Legendre, at the outset, deduced a general equation from which all others were derived," etc. This is again explicity stated on page 161, where the equation is actually given with the annexed comment:

$$\text{"R}\,\delta r - \Sigma m \frac{d^2 s}{dt^2}\,\delta s = 0 \qquad (118)$$

This is the most general principle of mechanics, and M. Legendre made it the fundamental principle of his celebrated work on *Mecanique Analytique, which consists chiefly of a discussion of equation* (118)"!

Few of our readers will need to be informed that the *Mecanique Analytique* was not written by Legendre, but by Lagrange. Nor was any such equation discussed in the *Mecanique Analytique*, nor did the author of that masterpiece of analysis propose or attempt to base the science on any single formula. Almost the first words in the book are, "I propose to reduce the theory of that science [mechanics], and the art of resolving the problems which belong to it, to *general formulas.*" Professor Bartlett of West Point, about the year 1850, was the first to show that the "general formulas" of Lagrange could be deduced from the single equation which Professor Wood has coolly accredited to the author of the *Mecanique Analytique*, or rather to M. Legendre, ignoring not only Lagrange's work, but that also which has been done by Professor Bartlett to develop the science in this country.

But let us return to the preface, and ascertain in what certain sense Professor Wood's book is the reverse of "M. Legendre's celebrated work on *Mecanique Analytique.*" "This work" (his own), continues Professor Wood, "at first establishes the equation for motion due to a single force, and by adding principle after principle the most general equations become established. The latter method is the one by which every science is at first developed, and presents great advantages over the former in the course of instruction; but the former, as a method of pure analysis, cannot be excelled." With respect to all this, we have to say: First, the method by which the science of mechanics has been developed is not the best method to be pursued in a course of instruction, because the principal importance of its study is as an exercise of the intellect, since it is a perfect illustration of the nature of scientific reasoning; and in this respect its *logical* development is of the utmost importance. Secondly, the science has not been developed in the way indicated by Professor Wood, as a very cursory examination of its history will show.

It is not necessary to extend these instances of gross carelessness and inaccuracy further; indeed we should not have devoted this space and prominence to a review of the work were it not that Professor Wood has the reputation, and deservedly has it, of being a very able mathematician; he should appreciate the fact that a widely known name and a high reputation impose corresponding obligations. The errors and lack of precision which abound in this work might be passed over as excusable in the work of an unknown beginner, but they become quite unpardonable when they are the result of gross carelessness, we might almost say slovenliness, in a master like Professor Wood, who undoubtedly knows better. We understand that this work has been adopted as the text-book in a number of our colleges and schools, still another reason for impressing upon its author the necessity of revising it thoroughly and correcting it before a second edition is allowed to appear.

The prevailing inaccuracy of our scientific education is, we believe, largely due to the fact that the country is flooded with text-books in which the prevailing characteristics are careless and reckless statements. We want no more of them, and certainly expect none from so high an authority and experienced a mathematician as Professor Wood. R.

MISCELLANEOUS.

PHOSPHOR copper used in the preparation of phosphor bronze is prepared, according to Dr. H. Schwarz, as follows: A crucible is fettled with a mixture of bone ash, silicic acid, and carbon, granulated copper is laid in and covered with a quantity of the fettling mixture, and the whole is fastened down with a cemented cover. Soda and glass can be added to promote fusion. At a fusing heat, the silicic acid acts on the phosphate, the phosphoric acid is reduced and taken up as freed by the copper. On the occasion of an experiment, fourteen parts silicic acid were added to eighteen bone ash, and four powdered charcoal, four parts soda and four powdered glass, made up with a little gum water, for lining the crucible; the latter was closed, the copper put in, covered with the mtxture and melted at a red heat. The lining mixture was but little incinerated. The copper grains appeared grey red, well run, and entirely free from blister. On analysis the copper gave 0.5 to 0.51 per cent. phosphorus. Another experiment gave copper with 3.25 per cent. phosphorus.

M. BERT, of Milan, has for a long time been conducting a series of experiments with a view of determining what are the physio-logical effects of condensed air on the human system. Many engineering works of the day involve the necessity of workmen carrying on their operations under various pressures of air, to say nothing of the fact that the rise and fall of the barometer involve us all in changes differing only in degree from that which the diver experiences when he descends from the surface of the water to a depth of fifteen or twenty fathoms beneath it. M. Bert thinks that the influences of ordinary changes of air as indicated by the barometer are due exclusively to the varying quantities of oxygen contained in the air inhaled. The pressure of oxygen and the consequent amount of it which finds its way into the blood, he shows, depends upon the centesimal proportion of it in the air and upon the barometrical pressure, and he says that the mischief which operatives in air-tight dresses so often experienced from the density of the air they are compelled to breathe might be completely obviated if an atmosphere were composed of air and nitrogen nicely adjusted in proportion to the pressure under which the mixture was passed along the tubes.